PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON

SERIES A

CONTAINING PAPERS OF A MATHEMATICAL AND PHYSICAL CHARACTER.

VOL. LXXVII.

LONDON:

PRINTED FOR THE ROYAL SOCIETY AND SOLD BY
HARRISON AND SONS, ST. MARTIN'S LANE,
PRINTERS IN ORDINARY TO HIS MAJESTY.

JUNE, 1906.

LONDON
HARRISON AND SONS, PRINTERS IN ORDINARY TO HIS MAJESTY,
ST. MARTIN'S LANE.

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SERIES A. VOL. LXXVII

No. A 514.—January 3, 1906.

No. A 515.—February 8, 1906.

PROCEEDINGS OF

THE ROYAL SOCIETY.

Section A.—Mathematical and Physical Sciences.

REPORTS ON THE TOTAL SOLAR ECLIPSE OF 1905, AUGUST 30.

(Presented by the Joint Permanent Eclipse Committee of the Royal Society and the Royal Astronomical Society at a Special Meeting of the two Societies, October 19, 1905.)

Report of the Expedition to Castellón de la Plana, Spain.

By H. L. Callendar, M.A., LL.D., F.R.S., Professor of Physics, and A. Fowler, A.R.C.S., Assistant Professor of Physics, Royal College Science, South Kensington. (Received October 19, 1905.)

Part I.—By Professors H. L. Callendar and A. Fowler.

The expedition to Castellón de la Plana, Spain, was one of the series organised by the Joint Permanent Eclipse Committee, the expenses being chiefly defrayed from the Government Grant Fund. Though 23 miles south of the central line, Castellón was selected as the most suitable station after careful inquiries as to the local conditions and facilities had been made by the Vice-Consul, Mr. Edward Harker, who also rendered invaluable aid to the expedition in various other ways. The advantages of being near a town of considerable size sufficiently compensated for the loss of 18 seconds in the duration of totality as compared with that on the central line, and, for some of the work, the resulting change in the position angle of second contact would have been a distinct gain.

The party originally included Mr. W. Shackleton, but in consequence of a temporary illness he was unable to go out to Spain, and the expedition thus suffered the serious loss of an experienced observer at the outset. Mr.

Shackleton had, however, practically completed the large scale coronagraph and prismatic camera which he had intended to use, and it became possible to utilise this apparatus through the voluntary assistance of Mr. E. H. Rayner, of the National Physical Laboratory.

Mr. T. Banfield accompanied the expedition as technical assistant, and Messrs. Isaac Molloy, E. Cahen, and J. J. Steward joined the party as volunteers.

Messrs. Fowler, Banfield, Molloy, and Cahen arrived at Castellón on August 3, and were welcomed at the railway station by a deputation from the Corporation, headed by the Deputy-Mayor, Señor Don Francisco Campos, together with a number of prominent citizens, and Señor Don José Badia, private secretary to the Vice-Consul, who was to act as interpreter. Every possible assistance was promised by the local authorities.

After an inspection of the various sites which had been suggested, it was decided that the greatest facilities were afforded by the grounds of the new Provincial Hospital which is in course of construction. There was ample space enclosed by a high boundary wall, a good supply of cold water, skilled workmen on the premises, and an abundance of completed rooms which could be used as store rooms, photographic rooms, and so on. Permission to establish the camp on this site was readily granted on application to the Deputy-President of the Provincial Deputation, Señor Don Tiburcio Martin.

The work of clearing the ground, building piers for the instruments, and erecting huts was commenced without delay, under the supervision of Señor Don Francisco Tomás, a local architect, who generously performed his own part of the work gratuitously.

During the preparations the Military Governor (His Excellency General Don Juan Manrique de Lara), the Civil Governor (His Excellency Señor Don Sanchez Ortiz), and the Mayor of Castellón made constant inquiries, either personally or by messenger, as to the needs of the expedition; and, in addition, Señor Don José Marza, Town Councillor, was almost constantly in attendance at the camp to ensure that nothing was wanting to facilitate the work.

Professor Callendar arrived at Castellón on August 14 in time to make observations of the full moon; Mr. Rayner on August 17, and Mr. Steward on August 28.

Ten days before the eclipse the necessary drills were commenced and carried on every day at dusk, and also on the two days preceding the eclipse, near the time at which totality would occur.

The weather conditions during the 25 days preceding the eclipse were very

promising, there being only three or four days on which good observations would not have been possible. The day of the eclipse, however, was unsettled, and though the first and fourth contacts were observed in perfectly clear sky, not even a glimpse of the sun was obtained during totality. The clouds of early morning were dispersed by a shower of rain, which ceased in ample time for the final adjustment of the instruments, and for records of the prominences before the commencement of the eclipse. These hopeful conditions, however, only persisted for about 30 minutes after first contact, when a great bank of slowly-moving clouds approached from the north-west and completely obscured the sun until totality was at an end. About a minute after totality the crescent sun was seen for an instant, but the obscuration continued with very short breaks until about 20 minutes before the last contact, after which the sky was clear until near sunset.

No results were accordingly obtained during totality, except such as are given by the automatic records of solar radiation and temperature, to which separate reference is made later.

Through the kindness of the Military Governor, a guard of soldiers was stationed in and about the camp on the day of eclipse in order to prevent any possible interference with the work of the observers. It was particularly desired that crowds of people should not be permitted to collect in the vicinity of the camp so that the involuntary shouts to which the phenomena of totality are liable to give rise should not clash with the time signals regulating the exposures of the photographic plates. The arrangements made were entirely satisfactory.

In addition to those otherwise mentioned in this report, the thanks of the expedition are due to Señores Manuel Montesinos (Architect of the Hospital), Telmo Vega (Secretary to the Provincial Deputation), Miguel Peris, and Antonio Gomez, for valuable help in various ways, and to the owners of the numerous factories in the district who stopped work on the day of the eclipse so that there should be no smoke to mar the observations. Special mention should also be made of the invaluable services rendered to the expedition by Señor Badia, who by no means restricted his assistance to that of an interpreter.

Position of Camp and Times of Contact.

The precise geographical position of the eclipse camp was derived from the co-ordinates of a point at the summit of the City Tower as given by the Trigonometrical Survey of Spain,* namely:—

Latitude .... 39° 59′ 10″·02

Longitude .... 3° 39′ 0″·33 east of Madrid.

* 'Red. Geodesica de 1er Orden de España,' Madrid, 1894, p. 45.

The corrections to the camp were very kindly determined by triangulation, by the City Architect, Señor Don Godfredo Ros de Ursinos, with the following results:—

Latitude	39° 59′ 0″·75
Longitude	0° 2′ 47″·52 west of Greenwich.
„	0 h. 0 m. 11·17 s.

Adopting these co-ordinates, and calculating by the approximate formulæ given in 'Nautical Almanac Circular,' No. 19, the following data were found:—

	h.	m.	s.
Eclipse begins August 29	23	55	38 G.M.T.
Totality „ „ 30	1	16	32
Totality ends „ 30	1	19	58
Eclipse „ „ 30	2	35	57
Duration of totality...............		3	26

Angle from north point of 1st contact	295°	Towards east.
„ „ 4th „	114°	

Other calculations gave—

Angle from north point of 2nd contact	137°
„ „ 3rd „	273°
Sun's altitude at mid-totality	54° 40′

The beginning and end of totality, as observed in cloudy sky, were too indeterminate to permit useful records of the times at which they occurred, but Mr. Fowler carefully observed the times of first and fourth contacts by the spectroscopic method, with the following results:—

	h.	m.	s.
1st Contact, G.M.T.	11	55	14
4th „ „	2	35	52

The first contact was thus recorded 24 seconds, and the fourth five seconds, before the respective times calculated. The chronometer error adopted was the mean of two determinations made with a theodolite on the afternoon of the eclipse differing by only two seconds. A more accurate calculation of the times from the Besselian elements given in the 'Nautical Almanac' does not change the results by so much as a second, so that there remains a considerable discrepancy between observation and calculation.

Instruments and Observers.

The general programme of the expedition, which was somewhat extended in consequence of the voluntary assistance of local gentlemen, will be gathered from the following list of instruments and observers:—

20-*Inch Reflector*, with appliances for the measurement of solar and coronal radiation.

H. L. Callendar.

Absolute Recording Bolometer, for normal solar radiation, equatorially mounted.

Horizontal Bolometer, for recording vertical component of total radiation.

Recording Electrical Thermometers, for air temperature, etc.

H. L. Callendar.

Slit Spectrograph, adjusted for the region B to F.

A. Fowler

T. Banfield, Joaquin Garcés } Assisting with plate-holders, etc.

4½-*Inch Equatorial*, with Evershed solar spectroscope, for visual observations.

A. Fowler.

4-*Inch Coronagraph*, 38 *feet focal length*, with direct-vision prism for coronal and chromospheric spectra.

E. H. Rayner.

I. Molloy, in charge of prism.

Venancio Soto, José Babiloni } assisting with plate-holders.

3-*Inch Coronagraph*, 57 *inches focal length*, with green screen for inner corona, as suggested by Mr. Shackleton.*

T. Banfield.

3-*Inch Coronagraph*, 20 *inches focal length*, with small polar heliostat, for coronal extensions.

E. Cahen.

Spectrograph of small dispersion, with small cœlostat and image lens for spectrum of outer corona.

Francisco Betoret.

Slitless spectroscopes, for drawings of green ring.

A. Fowler, T. Banfield, Telmo Vega, Francisco Betoret.

2-*Inch Telescope*, for direct observation of corona in the region of a previously selected prominence.

J. J. Steward.

Thermograph and Barograph.

J. J. Steward.

* 'Monthly Notices, R.A.S.,' vol. 60, p. 433, 1900.

Observations of Stars during Totality.

I. Molloy.

Observations of Shadow Bands.

Juan Vilo.

Time Signals.

J. J. Steward, chronometer.

José Badia. }
Luis Giraudier } signals during totality.

Although the main objects of the expedition were frustrated by clouds, a more extended account of the principal instruments, and of the observations which it was intended to make, may possibly be suggestive on some points in preparing for future eclipses. With regard to the sections not dealt with further, it need only be mentioned that Capella was seen during totality through a break between clouds, and that the fall of temperature indicated by the thermograph was 6°·7 F., while from 30 minutes before to 11 minutes after totality the barograph registered a decided gradual increase of pressure, amounting to 0·02 inch, followed by a slight lowering.

Part II.—By Professor H. L. Callendar, F.R.S.

The 20-inch Reflector.

The mirror made by Common had an aperture of 20 inches and a focal length of 45 inches, giving an image of the sun approximately 1 cm. in diameter. The instrument, as received, had an equatorial mounting with a small slide-holder at the principal focus, and was used by Father Perry for taking photographs of the corona in 1889. In order to adapt it for measuring the heat radiation of the corona, for which purpose the large short-focus mirror was very suitable, the slide-holder was replaced by a diagonal plane mirror, projecting the image to the side of the tube in a convenient position for observation with an eye-piece or a sensitive bolometer or thermopile. The mounting of the telescope was somewhat rough, but after some adjustment of the bearings, it was found to be possible to set the telescope within two or three minutes of the position of any celestial object by means of the circles. As there was no provision for fine adjustment, the eye-piece fitting, carrying the thermopile, was mounted on a sliding plate with a rack-and-pinion movement of sufficient range in right ascension and declination, which proved extremely convenient for the purpose for which the instrument was required. The driving clock at first was very unsatisfactory, but after some alterations to the governor and

pinions, it was successfully adjusted to follow for nearly an hour without appreciable error.

The tube of the telescope was fitted with a diaphragm of 15 inches diameter, which limited the aperture, but improved the definition considerably. With this reduction, after allowing for obstruction by the flat, and for loss at the two reflections, the effective concentration of the rays at the focus was upwards of 1000 times, which was ample for the purpose. The mirrors were freshly silvered before packing, about the middle of July, and were necessarily somewhat tarnished by August 30. The loss of heat due to this cause was not serious, and would not have affected the results, as all the measurements were comparative.

The Absolute Recording Bolometer.

This bolometer was designed for the determination of solar radiation in absolute measure by the electric compensation method. The radiation admitted through a measured aperture of 3 sq. cm. was received on a blackened grid of fine platinum strips arranged in such a way as to intercept the whole of the admitted beam. The increase of resistance of the grid, which was nearly proportional to the intensity of the incident radiation, was automatically recorded by means of a Callendar Recorder of the usual pattern. The intensity in absolute measure was determined by observing the value of the electric current required to produce the same rise of temperature in the grid as the radiation to be measured. The bolometer was provided with compensators for eliminating the loss of heat by conduction at the ends of the strips and the effect of changes in the surrounding temperature on the resistance of the grid. The instrument was contained in a cylindrical water-jacket fitted with suitable diaphragms to protect it from air currents, and to limit the radiation received to a small part of the sky in the neighbourhood of the sun. When the instrument was exposed to the sun, a current of water was kept circulating through the jacket to prevent rapid or excessive variations of temperature, and the actual temperature of the water-jacket at any time was recorded by means of an electrical thermometer. The apparatus was mounted on the tube of the 20-inch reflector, and the openings in the roof of the hut were arranged to permit of continuous records being taken between the hours of 10 A.M. and 3 P.M., so as to include the whole duration of the eclipse.

Apart from its use for recording the variations of solar radiation, the instrument was intended for reducing to absolute measure the readings of the coronal thermopile. For this purpose the tube of the telescope was provided with a double cover of tin plate fitted with a series of small

apertures which could be uncovered at will so as to admit a known fraction of the full solar radiation to the coronal thermopile. By comparing the readings of the coronal thermopile with the simultaneously recorded readings of the absolute bolometer, it was easy to obtain a factor for reducing the readings of the thermopile at full aperture taken on the moon or the corona to absolute measure.

Incidentally a comparison was made between the readings of the absolute *bolometer and one of Angström's pyrheliometers.* The two instruments were *found to agree very closely in the relative values of the radiation over a wide range, but the readings of the bolometer were nearly 1 per cent.* higher than those of the pyrheliometer for a radiation of 1 calorie per square centimetre per minute. This may have been due to some accidental defect of the pyrheliometer, as the values given by the two strips differed by nearly 15 per cent., which appears to be unusual and excessive for this type of instrument.* It might also be explained by an error in the method of reduction, which does not appear to have received attention hitherto. In consequence of the increase of the resistance R of the strip with temperature, the heat C^2R, generated by the compensating current C, increases with rise of temperature, so that it is necessary to use different values of the reduction factor, which are tabulated for different temperatures as indicated by a small thermometer in the instrument. The thermometer, however, merely gives the temperature of the case, and not that of the strip, which must be many degrees hotter when the intensity of the radiation is so great as 1 calorie per square centimetre per minute. In the Angström pyrheliometer it is not easy to obtain the actual temperature of the strip under these conditions, but in the bolometer, the strips of which are of similar width, the rise is found to be upwards of 20° C. By assuming a similar rise of temperature in the pyrheliometer strips above that indicated by the attached thermometer, and employing the appropriate reduction factor, it is noteworthy that the readings of the pyrheliometer would be brought into closer agreement with those of the bolometer.

The Coronal Thermopile.

Previous observations by Langley and Julius had indicated that the heat radiation of the corona must be comparatively feeble, and that it would be necessary to employ the most delicate instruments to measure it with

* Mr. W. E. Wilson, F.R.S., kindly lent me a second Angström pyrheliometer, with which simultaneous comparisons were made. The two strips of Mr. Wilson's instrument differed rather less from each other, but the mean of the two gave a result 7 per cent. lower than the other Angström pyrheliometer.

certainty. Langley's observations* were made with a straight bolometer strip 1 cm. long and 1 mm. wide, receiving an image of a slit of the same dimensions placed tangentially to a solar image obtained with a siderostat and a mirror of 50 cm. diameter and 100 cm. focal length. But the aperture actually utilised on the corona was limited to 280 sq. cm. by a cat's-eye diaphragm. With this apparatus a negative deflection of 18 scale-divisions was obtained on the body of the moon, and 13 scale-divisions on the corona after eight reflections at silvered surfaces. The difference of 5 scale-divisions appears hardly sufficient to form a satisfactory basis of argument with regard to the nature of the coronal radiation. The objection to the straight slit placed tangentially to the solar image of less than 1 cm. diameter is that a comparatively small portion of the slit receives radiation from the inner corona. A greater effect might evidently be secured by making the bolometer strip in the form of a circular arc embracing the image. Two bolometers of this kind were accordingly made with circular strips, but otherwise of similar dimensions to that employed by Langley. It was found, however, that a current of only one-tenth of an ampère raised the temperature of the strip nearly 20° C., and the variations of zero due to the heating of the strip by the current were too large to permit the employment of a sufficiently sensitive galvanometer to give a deflection of the desired magnitude. Langley is stated to have employed a current of 0·2 ampère in his observations, which would make the heating effect four times as great. The degree of accuracy attainable with a bolometer is limited by the disturbance due to the heating effect of the current. For a given amount of energy expended in heating the strip, the steadiness, other things being equal, will be directly proportional to the surface available for dissipation of heat. Bolometers with a small receiving surface are, for this reason, necessarily less sensitive than large ones. When it becomes necessary to employ a very small receiving surface, as in the case of the corona, it is often preferable to employ the thermoelectric method.

The thermopile employed by Julius in his observations† on the corona in 1901 had a receiving disc 5 mm. in diameter directly exposed to the coronal radiation without the intervention of any mirrors to concentrate the rays or form an image. The thermopile was fixed at the bottom of a long tube with suitable diaphragms, and measured the total effect of the heat radiation from a region of the sky about 3° in diameter surrounding the sun. The differences between the scale readings obtained on the corona and on neighbouring parts of the sky during totality varied from 0 to 8 scale-divisions, but were rendered

* 'Astrophysical Journal,' vol. 12, p. 72, 1900.

† Published by the Eclipse Committee of the Royal Academy of Amsterdam.

uncertain by the continual passage of light clouds. The sensitiveness of the instrument was such that it would give a deflection of 23 or 24 scale-divisions (estimated) on the full moon, or the equivalent of about 2,000,000 scale-divisions on full sunshine at 1·25 calorie per square centimetre per minute. Assuming that the total radiation of the corona is of the order of one-millionth of full sunshine, one could hardly expect by this method to obtain a satisfactory measurement of its intensity.

The coronal thermopile, designed for the 20-inch reflector, had a receiving surface consisting of ten small blackened rectangles of very thin copper arranged on the circumference of a circle nearly fitting the image of the sun, so as to receive the greater part of the radiation of the inner corona. The copper rectangles formed the inner junctions of a series of thin bars of antimony and bismuth alloys arranged radially on a thin annular disc of mica. The outer junctions of the couples were formed by thin copper strips at the circumference of the mica disc. The pile was constructed in two halves of five couples each on opposite sides of the disc, and the two halves were connected either in series or opposition through a suitable switch to the galvanometer. The method of construction is shown in fig. 1, but the receiving rectangles were more evenly spaced than as shown in the diagram.

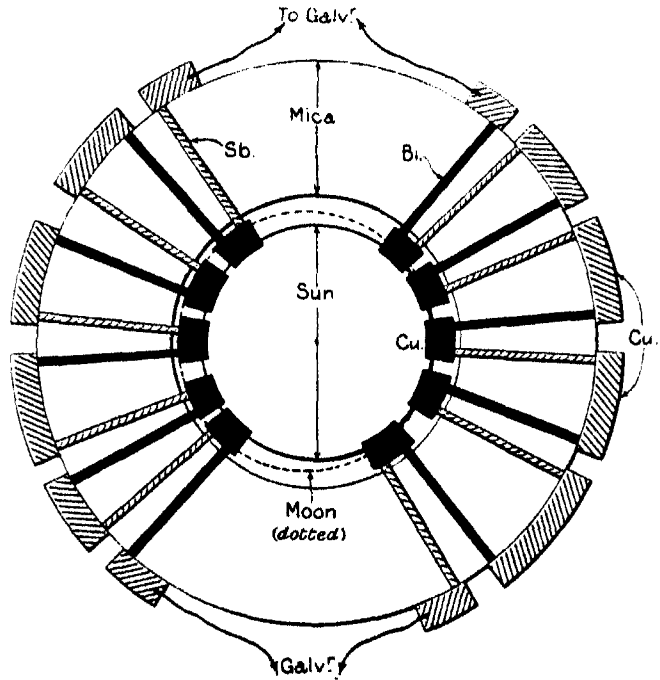
The mica disc carrying the thermopile was suspended in an ebonite ring by means of four thin connecting wires attached to terminals fixed in the ring. The ebonite ring carried on one side a tube sliding in the eye-piece fitting, by which the plane of the thermopile could be adjusted to coincide with the focal plane of the mirror, and on the other side a thick metal tube with diaphragms to screen the thermopile from draughts and from extraneous radiation. The diameter of the innermost diaphragm close to the thermopile was 14 mm., so that only the inner junctions were exposed to radiation. The end of the telescope tube was provided with a double tin-plate cover projecting all round beyond the sides of the tube in such a manner that when the tube was directed on the sun, the eye-piece was perfectly shielded from direct radiation.

Great care had been taken in constructing the thermopile to make all the elements of equal thickness and the copper receiving surfaces of equal area, so that the two halves of the pile might be as nearly equal as possible in sensitiveness and thermoelectric power. In order to test this, the two halves of the pile were simultaneously exposed to the same radiation. The deflection observed when the two halves were opposed was less than one-thousandth part of the deflection obtained when the two halves were connected in the same direction, the radiation remaining unchanged. This accuracy of compensation was very important for the method which it was proposed to

adopt. As a subsidiary test, the resistances of the two halves of the pile were measured, and found to be 4·56 and 4·62 ohms respectively. Exact equality of resistance was not essential, but the result is satisfactory as showing how accurately the mechanician, Mr. W. J. Colebrook, of the Royal College of Science, had succeeded in executing the design.

As a result of this accuracy of compensation, the zero of the galvanometer remained extremely steady even under the most trying conditions, with the

FIG. 1.—Coronal Thermopile.



Enlarged about 3 diameters.

telescope exposed to full sunshine and surrounded by unequally heated objects. There was never any difficulty in taking accurate observations, provided that the sun was not allowed to shine directly on the eye-piece. At night, and during totality, when the disturbing influence of the solar radiations was absent, it is hardly necessary to say that no trouble was experienced.

Methods of Observation.

The galvanometer employed with the coronal thermopile was of the movable-coil type with a plane mirror, 1 inch in diameter, reflecting the image of a transparent millimetre scale at a distance of 3 metres into

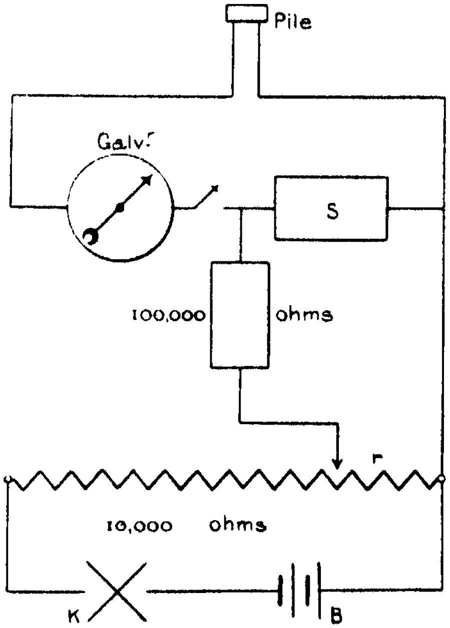
a telescope of 2 inches aperture and 3 feet focal length. With this power the definition was so good that it was easy to read to a tenth of a millimetre with certainty. The galvanometer was supported on a pier south of the telescope and proved extremely steady. The zero seldom shifted by more than a small fraction of a millimetre in the course of the day. The suspension was of very fine phosphor bronze, giving a deflection of 5 cm. nearly for one microvolt with the thermocouple in circuit, so that it was possible to read to 0·002 of a microvolt. A higher degree of sensitiveness, measured merely in scale-divisions per microvolt, might, no doubt, have been obtained by using a suspended magnet galvanometer with a small mirror. But it would not have been possible to obtain so good optical definition or equal steadiness of zero, especially in close proximity to the moving iron tube of the equatorial. It is doubtful whether any increase of accuracy could have been secured by using a more sensitive galvanometer; and it is certain that the trouble of taking the observations would have been greatly increased by the incessant variations of zero and changes of sensitiveness of the suspended magnet type.

In taking observations with the thermopile it was possible either (1) to read the deflection of the galvanometer, or (2) to compensate the galvanometer deflection by introducing an opposing E.M.F. of known value into the circuit. A preliminary test of the apparatus with the thermopile directly exposed to radiation of known intensity, as measured by the absolute bolometer, showed a deflection of nearly 25 cm. for one-thousandth of a calorie per square centimetre per minute, so that radiation one-millionth of full sunshine could be detected with certainty without using a mirror. When the pile was placed in the focus of the telescope, radiation one thousand times smaller than this could be observed, so that even if the intrinsic heat radiating power of the inner corona were only one ten-millionth part of the solar surface, it could still be measured to within 1 per cent.

The direct-deflection method was only suitable for small intensities of radiation. Even for observations on the moon the deflection obtained was far beyond the limits of the scale, and it became necessary to use the compensation method (2). This method, though admitted to be the most accurate, is generally regarded as being too slow and cumbrous for quick work at high pressure, as during totality. It was found, however, that, by a suitable arrangement of apparatus, quicker readings could be secured by the compensation than by the deflection method. The arrangement adopted is shown in the diagram, fig. 2. A battery, B, of known E.M.F., E, which was verified at intervals and found to be extremely constant, sends a steady

current through a resistance of 10,000 ohms arranged as a potentiometer, so that any convenient fraction r could be tapped off by revolving pointers. The ends of the resistance r were connected through a resistance of 100,000 ohms to a small resistance s in the circuit of the galvanometer and thermopile. The current through the 100,000-ohm circuit would be $Er \times 10^{-9}$, and the P.D. on the small resistance s would be $Ers \times 10^{-9}$, when there was no current through the galvanometer. If r and s were

FIG. 2.—Diagram of Electrical Connections for Compensation Method.



100 ohms each, and E was 3 volts, the P.D. introduced into the galvanometer circuit would be 30 microvolts, correct to about one part in 1000. The resistances s and r could both be varied from 1 to 1000 ohms, giving a range of $\pm$ 1000 microvolts.

The obvious advantages of this method, as compared with the usual bridge-wire method, are quickness of manipulation and avoïdance of errors due to variation of resistance at the sliding contacts. A more important advantage for thermoelectric work is that the sliding contacts are all in the battery circuit, where there is a relatively large electromotive force, so that accidental thermal effects, due to exposure of the working parts, or sliding friction, or the warmth of the hand, do not affect the galvanometer.

Observations on the Sun and Moon.

Observations were made at frequent intervals on the sun, for the purpose of testing the apparatus, and to serve as data for comparison with simultaneous readings of the absolute bolometer. As an example, when the record of the absolute bolometer showed 1·02 calories per square centimetre per minute for direct sunshine, the same sunshine admitted through a measured aperture 3 mm. in diameter in the cover of the telescope, and adjusted so as to fall on one half of the coronal thermopile, gave an E.M.F. which required the resistances $r = 352$, $s = 100$ ohms to be inserted to balance it. When the sun's image was shifted on to the other half of the pile, and the battery reversed, the balancing resistance required was $r = 348$ ohms. Shifting back again to the other half gave $r = 351$; the resistance s remaining unaltered. Adding the effects observed on the two halves of the pile, and taking $E = 2·91$ volts, we find a thermo- E.M.F. of 204 microvolts produced in the coronal pile by a known fraction of the solar radiation of known intensity.

The apparatus was erected in time to get some observations with the coronal pile on the full moon on the night of August 16, shortly before the partial eclipse. This was useful as a test of the sensitiveness of the apparatus and of the method of working adopted. The most essential point in such observations is to eliminate the variable effects of atmospheric radiation, for which the differential method of observation with the two halves of the pile appeared particularly suitable. Using the full aperture of the telescope, and exposing first one half of the pile and then the other to the lunar image by means of the rack-and-pinion motion of the sliding-plate on which the pile was mounted, with a resistance $s = 10$ ohms in the galvanometer circuit, the balancing resistance, r, was found to be nearly constant with a mean value of 290 ohms. The E.M.F. of the battery, E, being 2·92 volts, this was equivalent to a thermo-E.M.F. of 17·0 microvolts for the whole pile, as compared with 204 microvolts obtained on the sun with an aperture of 3 mm., as described in the observation already recorded. This makes the radiation of the full moon, neglecting atmospheric absorption, as in the case of the sun, about 6·6 micro-calories per square centimetre per minute, or about 1/150,000 of that of the sun. The compensation for atmospheric radiation was found to be very perfect, and the sensitiveness ample, as it would have been possible to detect radiation six thousand times smaller than that of the moon. As a further test of the accuracy of compensation for atmospheric radiation, a series of similar readings were taken *in full daylight* at 7 A.M. on the planet Jupiter. These gave a difference of

one-fifth of a millimetre deflection in favour of the planet, which could not of course be regarded as a measurement, but illustrates the practically complete elimination of atmospheric effects.

In taking observations on the corona it was intended to apply a similar method, making use of the motion of the moon during totality to define the exact area of the corona corresponding to the differential reading. At the commencement of totality, the thermopile being centred on the sun as indicated in fig. 1, the inner corona on the eastern limb would be fully exposed, while on the western it would be partly covered by the moon, as indicated by the dotted circle. At the end of totality the reverse would be the case. The difference of the readings would correspond to the radiation of the strip of the inner corona uncovered by the motion of the moon between the two readings. The area of the strip of corona considered could be accurately determined from the times at which the readings were taken. The advantage of this method is that it accurately compensates for external disturbances, in addition to giving the radiation from a definite area. It was intended to take as a standard of comparison of similar shape the radiation of the solar crescent a few minutes after and before totality. It would also have been possible to take observations of the total radiation of the corona intercepted by the pile at the middle of totality by connecting the two halves of the pile in the same direction instead of in opposition. But in this case it would have been necessary to take an additional reading with the pile directed to a neighbouring part of the sky to determine the effect of atmospheric radiation, as in the methods adopted by Langley and Julius, and the elimination of atmospheric effect could not for many reasons be regarded as being so perfect.

A number of other readings and comparisons were taken during our stay, on the sun and moon, but the examples already given will suffice as illustrations of the method.

The Horizontal Bolometer.

The horizontal bolometer was of the usual type designed for recording the vertical component of sun and sky radiation. It consisted simply of a pair of platinum thermometers wound on a horizontal mica plate fixed in a sealed glass bulb. One of the thermometers being coated with black enamel is raised to a higher temperature than the other by exposure to radiation. The difference is very nearly proportional to the intensity of the radiation, and is automatically recorded on an electrical recorder of the usual type. It is, of course, necessary for an instrument intended to be exposed in all weathers that the surface receiving radiation should be protected by a glass bulb. It

has often been objected that this will cause a very serious error in the record, since glass transmits only a small fraction of the radiation. In practice, however, it appears that this source of error compensates itself. The glass becomes heated and radiates to the enclosed bolometer in practically the same proportion as its absorbs.

This was tested in a very simple manner by making comparisons between the horizontal bolometer enclosed in its glass bulb and the absolute bolometer with the naked strips directly exposed to the same radiation. When the quality of the radiation was varied over a very wide range from a dull-red heat to the highest temperature attainable with an incandescent lamp, it was found that the ratio of the readings of the two instruments remained constant within the limits of error of measurement, showing that the selective absorption of the glass did not materially affect the result. When exposed to the sun and sky the records are not, however, exactly comparable, because the horizontal bolometer takes the vertical component of the total radiation, and measures the whole heat received by a horizontal surface, whereas the absolute bolometer, when equatorially mounted so as to be normal to the sun's rays, records the normal component and receives only a small part of the sky radiation from a region immediately surrounding the sun. The full sky radiation may often amount to 30 or 40 per cent. of the whole vertical component, according to the state of the sky and the altitude of the sun.

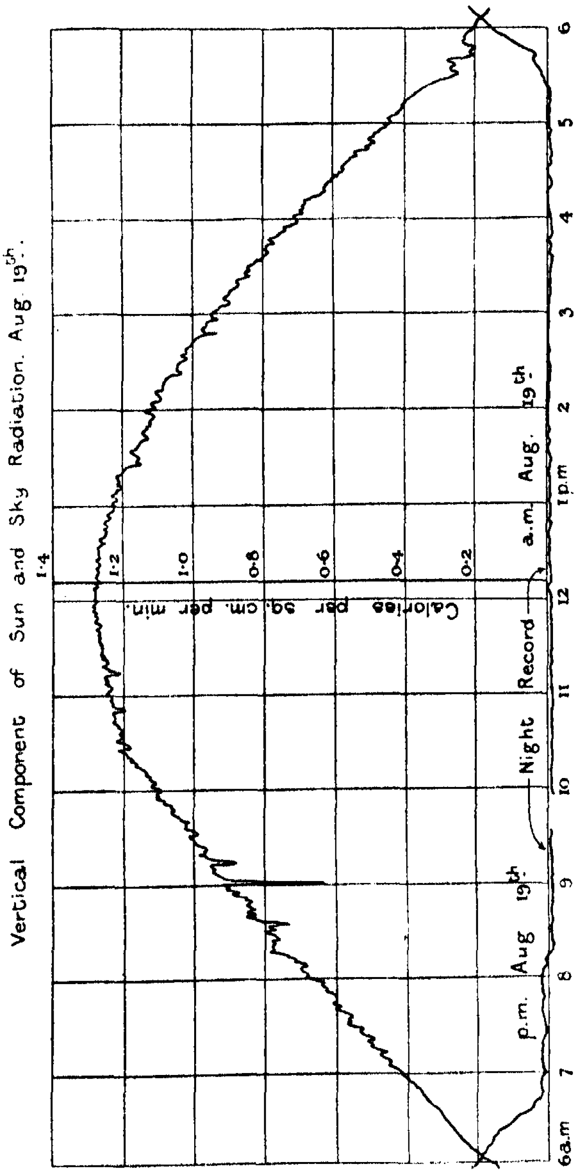
Description of the Records obtained during the Eclipse.

Although the sky was not clear during the eclipse, a description and reproduction of the records obtained may not be without interest, as it is the first time that an attempt has been made to obtain records of radiation and temperature on so large a scale. The record of the Vertical Component obtained with the horizontal bolometer on August 19, reproduced in fig. 3, illustrates the type of curve obtained on a clear day with a sky practically free from clouds. There are always small, incessant variations of radiation, even on the clearest day, which make it necessary to use a recording instrument if comparative results of any accuracy are required.

Fig. 4 shows the record obtained with the same instrument on the day of the eclipse. The heavy shower of rain which occurred about 8.40 A.M. was practically the only rain which fell during our stay. The sky cleared shortly afterwards, and the sun remained clear for half an hour after first contact. When it was obscured, the radiation fell from 1·08 cal. to 0·38 cal., the latter reading showing that the radiation from the cloud was at that time about 35 per cent. of the whole. While the sky remained clouded, the radiation

gradually fell to a minimum during totality, and then rose with occasional breaks in the curve, due to variations in the cloud bank. The sun reappeared in time for the observation of the last contact.

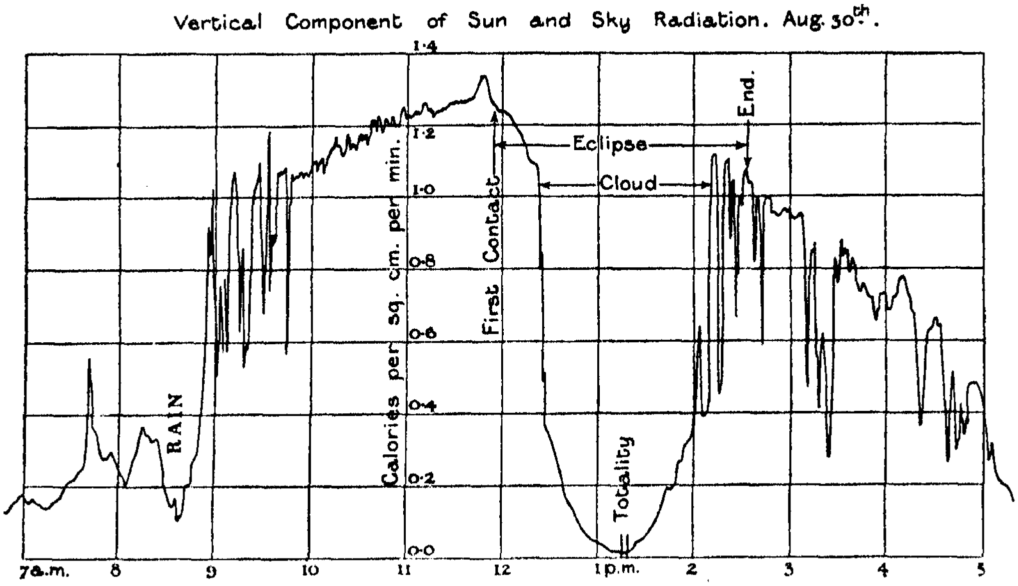
Fig. 3.



Reduced to half size of original record.

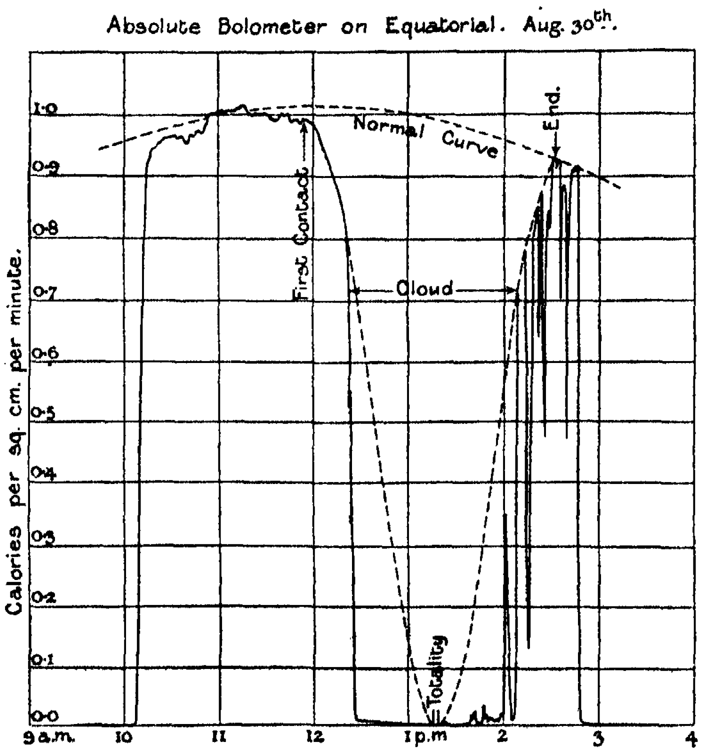
The record with the absolute bolometer shown in fig. 5, extended only from 10.10 A.M. to 2.50 P.M. The normal curve which the record should have followed on a clear day without eclipse is indicated by the dotted line. The record began to fall regularly at first contact, following the predicted

Fig. 4.



Reduced to half size of original record.

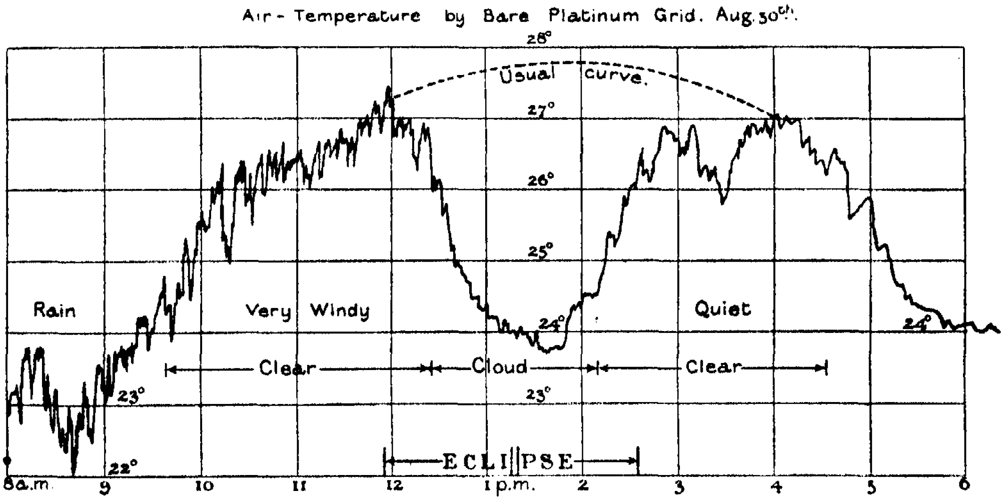
Fig. 5.



Reduced to half size of original record.

curve. As soon as the sun was obscured by clouds the reading fell very nearly to zero, because the aperture of the instrument includes only a very small part of the sky surrounding the sun. The dotted curve indicates the course which the record should have taken if the sky had been clear.

Fig. 6.



Reduced to half size of original record.

The record of air-temperature shown in fig. 6 was obtained with a bare platinum wire wound on an open mica frame, as designed for Brown's experiments on the temperature of leaves and air-currents.* The platinum grid was suspended at a height of 4 feet in a specially-designed screen, 1 metre cube, with a double top and free ventilation. This type of thermometer is extremely sensitive and free from radiation error. The scale was 2 cm. to the degree centigrade, and illustrates very well the incessant fluctuations, which are missed altogether by the usual type of thermograph.

Part III.—By A. Fowler.

The Spectrograph.

The special object of the work with the slit spectrograph was to photograph the spectra of the corona and chromosphere in the less refrangible parts of the visible spectrum, for which purpose, in consequence of the possible

* 'Roy. Soc. Proc.,' January, 1905, p. 124, vol. B. 76, where a figure is given showing the details of construction. A similar thermometer was made shortly afterwards, with slight differences of detail, to the design of Mr. E. H. Wade, of the Survey Department, Cairo, and has been employed at Helwan Observatory with very satisfactory results.

difficulty of obtaining plates highly sensitive to red light, it was considered advisable to use a prismatic spectrograph of large aperture.

The instrument employed was of the Littrow type, having an object-glass 3 inches in diameter, one prism of 60° and two of 30°, one of the latter being silvered on the back and adjusted so as to return the light through the prisms and object-glass into the camera. The slit was attached to the side of the camera, and light passing through it was thrown on the object-glass by a small totally-reflecting prism. The camera was provided with a set of multiple plate-holders, kindly lent by Sir Norman Lockyer, and the exposures were made by turning a hinged shutter in front of the plate from a vertical to a horizontal position.

This form of spectrograph is very compact, and a high dispersion is economically obtained. With the instrument in question, the linear dispersion from C to F was $5\frac{1}{2}$ inches, and with the slit set to $m = 2$ on Newall's "diffractional indicator" scale, the actual purity of spectrum realised on the photographic plates was about 13,000 in the neighbourhood of the green corona line.

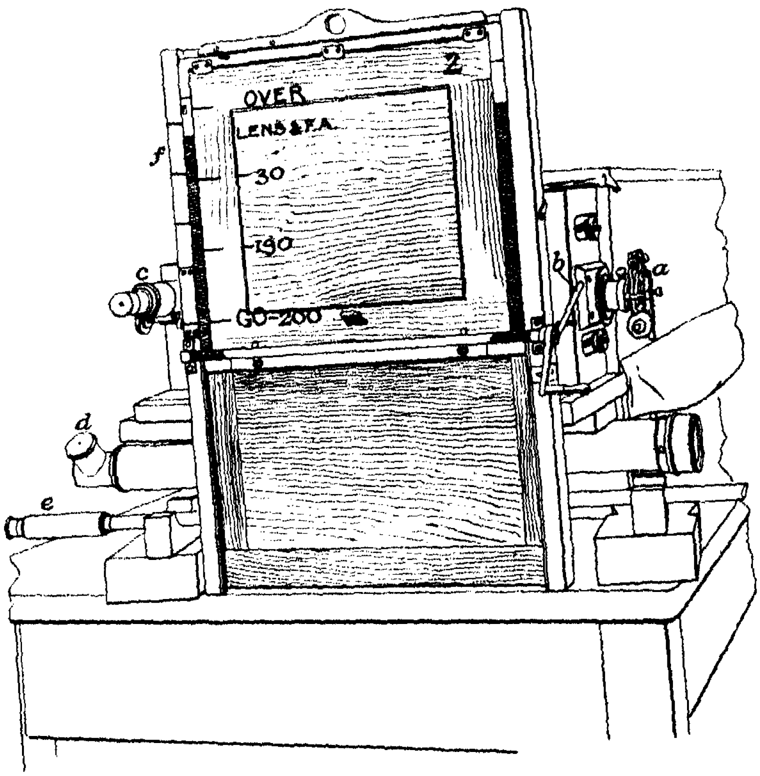
The spectrograph was used in conjunction with a 12-inch cœlostat, and the image on the slit was produced by a 6-inch objective of 76 inches focal length. The spectrograph itself was supported horizontally on a large packing-case inside a hut, while the object-glass and cœlostat rested on piers outside. The point of second contact in the image formed after reflection from the cœlostat was almost exactly at the extremity of a horizontal diameter, so that the flash spectrum would be depicted under the most favourable conditions, providing that the image could be kept tangential to the sun's limb.

To facilitate the working of the combination, two finders were attached to the spectrograph, one giving a direct view of the sun as seen in the cœlostat mirror, and the other utilising the spectrum reflected from one of the prism faces. The eye-piece of the latter was close to the camera, a mirror being introduced to send the light in the required direction. This arrangement was invaluable, as by its aid one could observe the exact counterpart of the spectrum presented to the photographic plate, and by means of a long rod attached to the fine adjustment of the cœlostat, the position of the sun's image on the slit could be controlled without removing the eye from the finder. No difficulty was accordingly anticipated in photographing the flash spectrum, even with the slit tangential to the sun's limb at the point of contact.

After many trials, made both in England and Spain, the plates selected for use during the eclipse were "Seed" plates sensitised for the visible spectrum by soaking for four minutes in a bath of pinachrome and, after washing in

distilled water, drying as quickly as possible. Very promising results were obtained with these plates in experiments on photographing the brighter chromospheric lines before the eclipse.

Fig. 7.—The Slit Spectrograph.



a. Slit.
b. Handle for operating exposing shutter.
c. Finder for viewing spectrum reflected from prism face.
d. Finder for viewing sun in cœlostat mirror.
e. Rod for fine adjustment of cœlostat mirror.
f. Repeating back and dark slide.

The exposures for totality were planned as follows :—

(1) "Go" to 200... For flash spectrum.
(2) 198 „ 190... For upper chromosphere.
(3) 188 „ 30... For coronal spectrum, including brighter chromospheric lines for determination of positions.
(4)* 15 „ "Over" For flash spectrum.

* Image readjusted between 3 and 4.

It was also intended to make 20 additional exposures on the cusps at intervals before and after totality, but the state of the sky was entirely unfavourable even for this part of the programme.

As a general remark, it may be mentioned that the constantly changing azimuth of the reflected beam of sunlight coming from the cœlostat caused a great deal of labour, which might have been avoided if a siderostat had been available for the work. The whole spectrograph and the image lens had to be readjusted, at least once a day, in order to maintain full illumination of the prisms, and in a temporary observatory this was a matter of considerable difficulty. Moreover, the cœlostat has the additional disadvantage that there is only one fine adjustment, moving the image in a direction inclined to that of the slit, so that, in passing from one limb of the sun to the other, it was necessary to displace the object-glass as well as to use the fine adjustment, With a siderostat, on the other hand, the spectrograph might be collimated once for all, and any desired part of the sun brought on the slit by means of the mirror adjustments.

Spectroscopic Observations.

To supplement the photographic work, it was arranged to make visual observations of the spectrum of the corona during the long exposure with the spectrograph, and, after totality, to repeat the interesting observations made by Sir Norman Lockyer during the eclipse of 1882.*

The instrument provided for these observations was an Evershed solar spectroscope of high resolving power, adapted to an excellent 4½-inch Cooke equatorial, which was very kindly placed at the disposal of the expedition by Mr. Shackleton.

The complete series of observations contemplated was as follows :—

Before Commencement of Eclipse.—To record the appearances and positions of the prominences.

At Beginning and End of Eclipse.—To determine the times of first and fourth contacts by the spectroscopic method.

Shortly Before Totality.—To observe the spectrum of the large spot near the east limb, in order to determine the effect of reduced sunlight, in connection with Evershed's suggestion, that the majority of unaffected lines in the spectrum of a spot may be due to diffused photospheric light.†

During Totality.—(1) To examine the "continuous" spectrum of the corona, especially near b, with the view of obtaining further information as to the "ribbed" structure noted by Sir Norman Lockyer in 1882.

* 'Roy. Soc. Proc.,' vol. 34, p. 296.

† 'Astrophysical Journal,' vol. 5, p. 248.

(2) To observe the structure of a portion of the inner corona as seen with a wide slit in the light of the green line.

(3) To search for the coronal lines between D and the green line suspected by Young and Harkness, and also to search for indications of iron lines.

After Totality.—To note how long the green line could be seen after the end of the totality, and to investigate the relative brightness and extensions of the arc and enhanced lines of iron at the cusps.

In addition to the above, half a minute was to be devoted to sketching the green coronal ring as seen with a direct-vision spectroscope of considerable dispersion, the collimator of course being removed.

On account of the unfavourable weather, only a very small part of this programme was actually carried out: namely, the determination of the times of first and fourth contacts, details of which have already been given, and the observation of the prominences before the eclipse commenced.

The large group of prominences on the eastern limb, reported by all observers who were favoured with clear sky, was well seen between 10.35 and 11.5, but even more interesting was a small intensely bright metallic prominence on the western limb, at a position angle recorded as 306°, counted from north through east. In the spectrum of this prominence, the *b* and D lines were exceptionally bright, as were also a great number of other lines ordinarily seen in such eruptions. The observations may possibly be of interest in investigations of the coronal structure in this region, and in case of error in the determination of absolute position angle, it may be useful to add that the eruption was about 149° from the middle of the large group, reckoned through the north point. Several other prominences were also observed, but they were mostly small and not very bright. As a general remark, it was noted that the whole chromosphere appeared to be considerably disturbed, and reversals of the D lines were observed in unusually high solar latitudes.

It may be added that considerable activity was shown in the large spot near the east limb. The C line was reversed and distorted in several places, and D_3 was distinctly visible as a dark line in the neighbourhood of the spot. On the day after the eclipse similar appearances were again seen at C and D_3, and the D and *b* lines were clearly reversed over the umbra.

Part IV.—1. By W. SHACKLETON, A.R.C.S., F.R.A.S.

40-foot Coronagraph and Prismatic Camera.

Originally it was intended that the coronagraph used by Mr. Maunder in Mauritius* should form part of the equipment of the expedition, but

* 'Roy. Soc. Proc.,' vol. 69, p. 256.

towards the middle of May Mr. Maunder received an invitation to join the Canadian expedition to Labrador, and it was agreed that he should again have the use of this coronagraph.

The replacement of an instrument for this work at so late a date was a serious embarrassment, but fortunately the expedition was helped out of the difficulty by the generosity of Dr. Copeland, who kindly placed at our disposal the 40-foot lens and large direct-vision prism which he had successfully used in 1898 and 1900.

It was decided to use the instruments according to Dr. Copeland's plan, viz., to use the lens alone as a coronagraph during the greater part of totality, and conjointly with the prism as a prismatic camera near the beginning and end of totality.

Instead of pointing the instrument to the sun or using a stationary mirror, however, it was proposed to employ a cœlostat and keep the camera horizontal, using stationary plates.

The equipment therefore consisted of—

(i) A Dallmeyer lens 38 feet 6 inches focal length of 4-inch aperture.

(ii) A D.V. prism, direct for λ 3890, of about 4-inch clear aperture. This was mounted on a platform in front of the objective, and could be inserted or withdrawn as required.

(iii) A 12-inch cœlostat.

(iv) A camera for use with above, as described below.

In order to provide for the daily change in azimuth of the reflected solar beam from the cœlostat, it was arranged that the objective and plate-holder should be connected together by a rigid camera body, in order that the two might be moved sympathetically.

This body was built up of tapered lattice girders (of mild steel) for sides, with similar lattice bracings top and bottom; the whole unbolted into four flat pieces for transport.

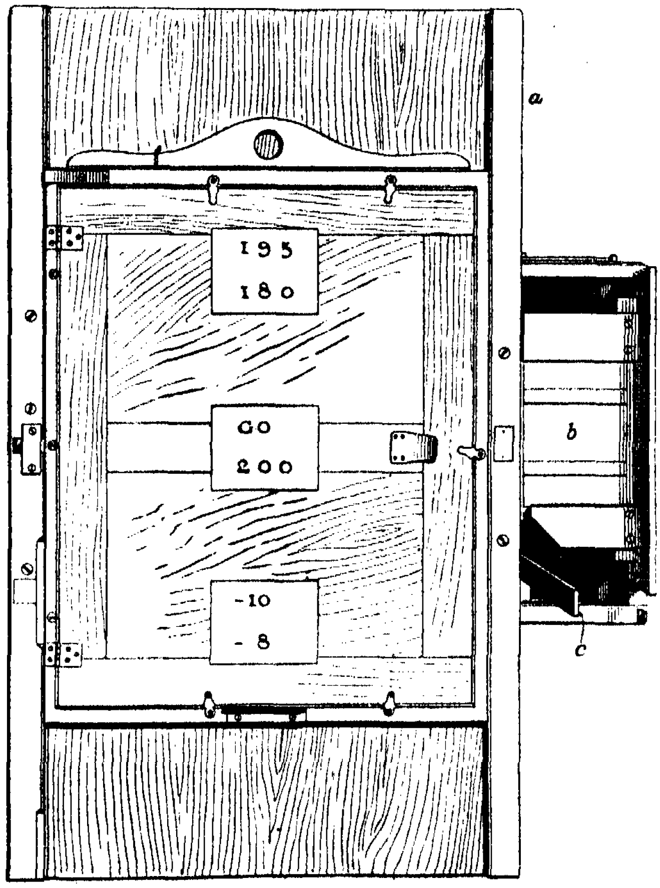
Each end of this skeleton tube was lined with a mahogany box for a distance of about 4 feet. The one at the narrow end carried the object-glass, immediately in front of which was the platform for supporting the prism. At the wider end an adjustable sliding box, divided into a large and small chamber by an inner partition, fit into the outer mahogany lining; a repeating back, into which the plate-holder fitted, was attached to the larger of these chambers, whilst the smaller chamber was provided with ground glass at the camera end, the other end being open to the object-glass; the use of this latter portion is explained below.

For making the exposures a balanced double-flap shutter was designed;

this was contained in the larger chamber, a few feet in front of the plate, and was operated by a lever.

As explained previously, it was intended that the prism should be drawn in front of the object-glass at the beginning and towards the end of totality,

FIG. 8.—Camera end of 40-foot Coronagraph and Prismatic Camera.



a. Repeating back and dark slide.
b. Ground glass screen for observation of less refrangible part of spectrum.
c. Handle for operating exposing shutter.

when photographs of the coronal and flash spectra were to be taken on plates 6 × 15 inches, the longer edge being in the direction of dispersion. This length of photographic plate was only sufficient to include the spectrum from about H_β to some part in the ultra-violet, and the visible portion of the spectrum fell on the ground glass attached to the small chamber on the right of the plate-holder; hence the spectroscopic phases of the eclipse

could be visually observed and simultaneously photographed; by this means it was anticipated that the flash spectrum might be secured both at the beginning and end of totality. During the greater part of totality the prism was to be removed, and photographs of the corona obtained on plates 12 x 15 inches.

It is only to be regretted that the weather proved unfavourable, and that the apparatus above described had not an opportunity of being put into use and its novel points tested.

2. By E. H. Rayner, M.A.

When I arrived at Castellón, on August 16, the cœlostat had already been adjusted and the piers built for the tube, but two days were occupied in adapting the exposing shutter and fitting a ground glass for the visual observation of the red end of the spectrum as designed by Mr. Shackleton. It was decided to use a ground glass rather than an eye-piece, as it would not require such close observation, and would allow a greater part of the spectrum to be seen. Horizontal lines were drawn on the ground glass at the limits of the continuous spectrum as calculated for 60, 20, and 10 seconds before totality, to serve as time signals for the whole camp. To accurately adjust these lines to the spectrum, the frame holding the ground glass was provided with a vertical motion and the necessary clamping screws.

Another signal at 10 minutes before totality was to be observed on the direct image of the sun, for which purpose a circle of the same diameter, 4 3/16 inches, was drawn on another piece of ground glass, and the angular extent of the cusps, 178°, marked upon it. This could have been superposed on the sun's image, and the time so obtained.

To render the camera light-tight, 60 yards of black sateen were wrapped in a spiral round the frame, so that there were two thicknesses everywhere. This was found quite satisfactory. In addition, a wooden frame was built over the tube to carry canvas sheets, in order to keep off the direct rays of the sun.

Considerable difficulty was experienced with the cœlostat clock, the sun's image in the camera having at first an oscillating motion of ¼ inch every two minutes, corresponding with the period of the driving screw. As the result of some days' work, however, this was reduced to about 1/30 inch. To produce this improvement, the socket in the worm wheel on the driving screw spindle was filed on one side and packed with a small piece of metal on the other, and both worm and driving screw had to be pressed so tightly into the parts driven by them that the clockweight had to be doubled. The further diminution of the periodic error which appeared, without anything

being done except the running of the clock in the usual course of trials, may probably be attributed to the action of dust in grinding the parts to a better fit.

"Squaring-on" the objective was very easily and quickly done by observation at the camera end of the images of a small electric torch reflected from the various surfaces of the lens.

Visual focussing of the sun on the ground glass was found to be greatly facilitated by keeping the ground glass in motion. By this means fine detail, such as the components of a dispersing sun spot, which were otherwise completely invisible on the ground glass, became very useful for focussing upon. This was in fact the only practicable method of focussing on the sun's image with the apparatus provided.

Attempts were made to obtain the actinic focus by direct exposure of very slow plates on the sun, but the results were unsatisfactory in consequence of over-exposure and air tremors, and recourse had to be made to photographing a star. The brightest that could be projected into the camera was γ Aquilæ, and the results showed a close agreement between actinic and visual focus.

A thorough investigation of any difference between visual and actinic focus of such a lens might usefully be made before the departure of an expedition, as a temperature change of some 3 inches in the focal length necessitates visual focussing just before exposure, with any allowance that may be required for difference between the two foci. The definition of the lens and mirror combined left nothing to be desired, and would well repay the greatest care taken with the mechanical adjustments.

It is also very desirable that the interesting point of change of focus with temperature should be fully investigated.

It may be useful to put on record the exposures which were decided upon. They are given in "eclipse times" in the following table, the duration of totality being 205 seconds.

Number of plate.	Begin.	End.	Remarks.
1	−10	− 8	For spectrum.
2	"Go"	200	"
3	195	180	"
4	170	150	Prism removed for corona.
5	140	85	" "
6	75	65	" "
7	55	25	Prism inserted for spectrum.
8	22	15	" " "
9	10	"Over"	" " "

Sir William Abney's intensity scales were impressed on the corona plates by the light of a standard candle on the night before the eclipse.

Mention should be made of the fact that had the clouds cleared away sufficiently soon to allow of photographs being taken during totality, valuable time would have been required to set the image properly in the camera, as the clock had no maintaining power to keep the image in position during the necessary windings.

The general design of the apparatus was very satisfactory, but a half-plate focal plane shutter, adapted for trial photographs of the sun for focussing purposes, would be a valuable addition.

Total Eclipse of the Sun, 1905, *August* 30. *Preliminary Account of the Observations made at Sfax, Tunisia.*

By Sir William Christie, K.C.B., Astronomer Royal.

(Received October 16, 1905.)

I. *General Arrangements.*

An expedition to observe the total solar eclipse of August 30 having been sanctioned by the Admiralty, it was arranged, in concert with the Joint Permanent Eclipse Committee, that a party from the Royal Observatory should make observations at Sfax, a town on the north coast of Africa, about 150 miles south of Tunis. The programme of observations consisted of photographs of the corona on various scales for coronal detail and streamers, and photographs of the spectrum of the corona and chromosphere.

The observers from Greenwich who took part in the expedition were Sir William Christie, Mr. Dyson, and Mr. Davidson. Professor Sampson, Mr. J. J. Atkinson, and Captain Brett, D.S.O., generously volunteered their assistance and shared the work of erecting and adjusting the instruments as well as of the observations on the day of the eclipse.

The Admiralty gave instructions that H.M.S. "Suffolk" should convey the observers and instruments from Malta to Sfax and should assist in the preparations and in the observations on the day of the eclipse. The expedition is greatly indebted to Captain Beatty, D.S.O., and to the officers and men of the "Suffolk" for their assistance and hearty co-operation.

We are indebted to M. Fidelle, Controleur and Vice-Consul (the representative of the French Government) and to the Mayor and Municipality for a

very kind reception at Sfax. Facilities were accorded for the landing of the instruments without customs examination; precautions were taken against the possibility of any inconvenience arising from the curiosity of the natives on the day of the eclipse, and any assistance we required, such as watering the ground to lay the dust, was readily given. Our thanks are specially due for the excellent site for the observations afforded us by the playground of the girls' public school being put at our disposition.

Itinerary.—It was considered that the observers and their instruments should arrive at Sfax not later than August 19, and it was hoped that the Admiralty might be able to make arrangements to convey them in a man-of-war from Gibraltar as the most convenient port for Sfax, it being advisable that the instruments should be conveyed directly by sea to avoid risks of overland travel and transhipments. This was not found practicable, but H.M.S. "Suffolk" was instructed to take them from Malta. In order to reach Malta in time, Sir William Christie, Mr. Dyson, and Mr. Atkinson, with the instruments and observing huts, had to leave London on July 29 by the P. and O. S.S. "Sumatra," arriving at Malta at 1 A.M. on August 7. The cases of instruments were transferred the same morning to the "Suffolk" and remained there till her departure for Sfax. The party of observers, including Professor Sampson who joined them on August 16, went on board the "Suffolk" on August 17, which, after a day's firing practice off Malta, left for Sfax on the evening of August 18 and arrived there at noon on August 19. The instruments were landed and carried to the site of the observing station the same day. Mr. Davidson and Captain Brett arrived on August 20, having come directly from England *viâ* Marseilles.

After the eclipse on August 30 the dismounting of the instruments was at once commenced. The same evening the undeveloped photographs were taken from the carriers and carefully packed. The cases containing the photographs and instruments were put on board the "Suffolk" on September 1, which sailed the same evening for Malta. At Malta they were transferred to a lighter, where they remained till September 8, when they were put on board the P. and O. S.S. "Formosa," which sailed for England early the next morning, arriving at Gravesend on September 17, the cases of photographs and instruments being landed at the Albert Docks on September 19, and brought direct to the Royal Observatory. The observers returned to England in different ways. Sir William Christie and Mr. Dyson accompanied the photographs and instruments, Mr. Davidson and Captain Brett returned as far as Malta on the "Suffolk," and Professor Sampson and Mr. Atkinson proceeded directly to England, *viâ* Tunis.

In connection with the travelling arrangements and carriage of the instruments thanks are due to the P. and O. S.S. Company, who allowed the instruments to be taken as passengers' luggage, free of charge, and also for the careful handling of the cases containing plates, lenses, mirrors, etc. We are also indebted to Admiral Bromley, Superintendent of the Malta Dockyard, for the facilities and care taken in the transhipments at Malta.

Station.—The station occupied was at Sfax, in Tunisia, and was situated some 10 or 12 miles north of the central line. In the choice of station we were assisted by Mr. Leadbetter, of Tripoli, and by Mr. Leonardi, the British Vice-Consul at Sfax. The French Authorities kindly placed the playground of the girls' public school at the disposal of the expedition, as the eclipse occurred during the school holidays. This playground was an excellent station, being of a convenient size, enclosed, and within a few minutes' walk of the hotel at which the observers stayed. In addition, on one side of the ground there was cover from the sun, which was useful during the unpacking and packing of the instruments, and during the putting together of the huts, and there were also the schoolrooms available for writing, etc., and storing plates, mirrors, etc., till they were required.

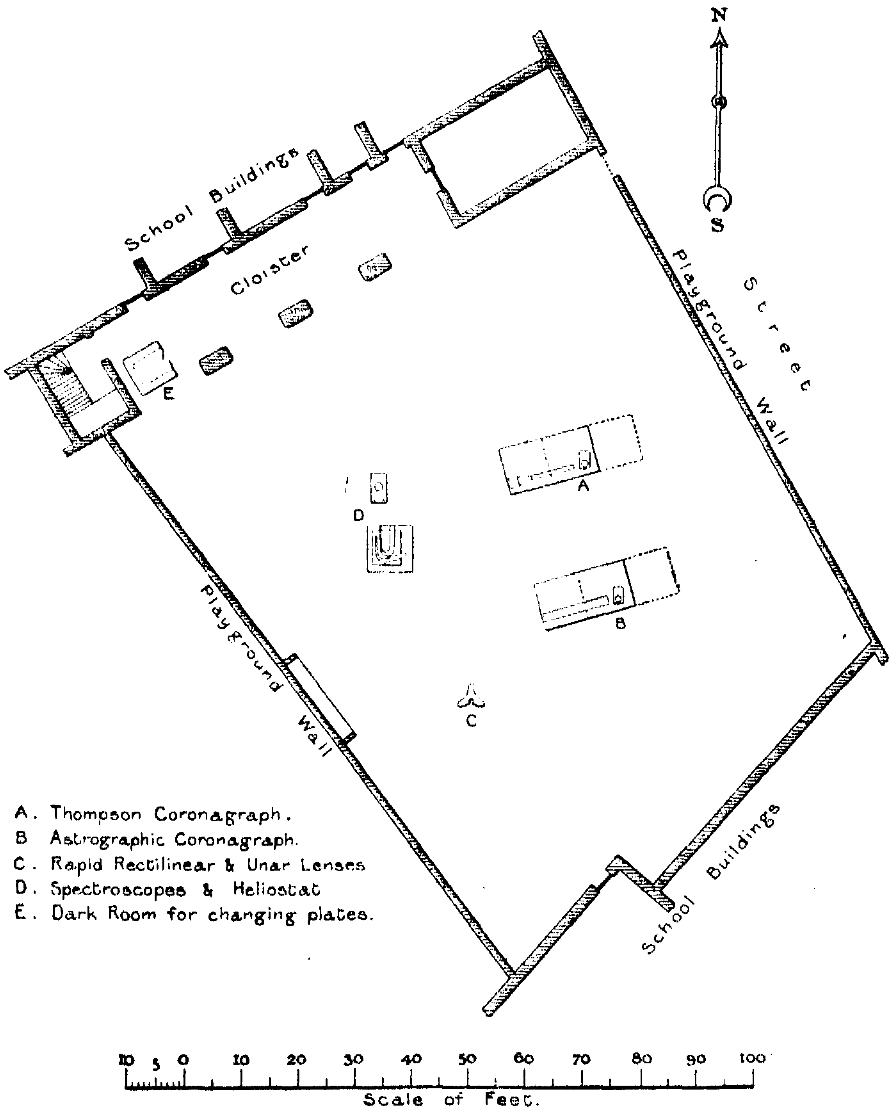
The latitude and longitude of the observing station are approximately :—

Long., $10^\circ\ 46' = 43$ m. 4 s. E. ; Lat., $34^\circ\ 40'$ N.

The accompanying plan shows the arrangement of the four instruments.

Erection of Instruments, etc.—As in the previous eclipses observed at Ovar and in Sumatra, the boxes in which the instruments were carried were utilised as stands on which to mount them. The boxes were weighted with 100 lb. practice projectiles lent by the "Suffolk."

The instruments were protected by light wooden framed huts covered with Willesden waterproof canvas. The huts were fitted together at Greenwich and the woodwork marked so that they could be erected quickly. The huts covering the Thompson coronagraph and the astrographic telescope were alike, and each consisted of two equal and similar parts, which could be bolted together. When the instrument was in use, the section of the hut over the cœlostat mirror was withdrawn a few feet, to give a clear view of the sun. Each section was 8 feet square and 6 feet high, rising to 8 feet at the gable. The canvas was stretched over the top and sides in two lengths, and tacked to the framework. The ends of the section covering the camera were fitted with canvas panels, which were removed during observations. The spectrographs were in a similarly constructed hut consisting of only one section 8 feet square, with panels at one end. The double tube mounted on the Dallmeyer photoheliograph stand, when not in use, was covered with a piece of canvas.



Plan of Eclipse Station at Sfax, 1905, August 30.

Personnel.—In the following list the names of those who took part in the observations are given :—

Sir William Christie.—Thompson coronagraph.

Mr. Dyson.—Spectrographs.

Mr. Davidson.—Astrographic telescope.

Professor Sampson.—Dallmeyer rapid rectilinear and Unar lenses.

Mr. Atkinson.—Heliostat of the spectrographs.

Captain Brett changed plates of flint spectrograph.

Commander Hyde.—Time observations.

Lieutenant Ballantyne changed plates of quartz spectrograph.

Lieutenant Hopwood made drawing of the corona.

Lieutenant Biddlecombe (R.N.R.) gave the exposures for Professor Sampson.

Mr. Eason gave the exposures for Mr. Davidson.

Two petty officers counted seconds with a metronome in turns for each minute during the totality. A petty officer also recorded the times of exposure for the Astronomer Royal. In addition, eight seamen, two at each instrument, assisted by handing the plate-holders to the observers.

The general arrangements for the observations were exactly similar to those made at Ovar and in Sumatra. The programme of observations was rehearsed on the afternoons of September 27 and September 29.

Day of the Eclipse.—Each day, till August 29, although there was sometimes a little cloud in the early mornings, the sky was invariably cloudless in the afternoons—the time of the eclipse. August 29 was cloudless, but there was a "scirocco," and the maximum temperature was 100° Fahr., which was several degrees higher than on previous days. On August 30 there were clouds at sunrise, which increased later with haze round the sun. This partly cleared away, but some haze remained, with passing clouds through the eclipse.

The inner corona was very bright all round the sun, but no great extensions or streamers were remarked. A drawing made by Lieutenant Hopwood on board the "Suffolk" shows the maximum extension visible to the eye to have been only 20′ from the sun's limb, though the photographs show much greater extension. The only star seen, although watch was kept by several of the officers of the "Suffolk," was Regulus.

Watch was kept on board the "Suffolk" for possible electrical disturbances during the eclipse which might affect the wireless telegraphy instruments. Lieutenant Ballantyne has furnished the following report:—

"The wireless telegraphy instruments of the 'Suffolk' were connected up and kept in sensitive adjustment for receiving from 8 A.M. on August 29 to 8 P.M. on August 31. The 'plain' and 'B' receiving jiggers were used in the receiving boxes, and the magnetic detector was also used for receiving.

"Observations were taken every two hours, and from 8 A.M. to 8 P.M. the instruments were under continual observation.

"No unusual atmospheric disturbances were observed during these three

days, and from 8 A.M. to 6 P.M. on August 30 the atmosphere was quite free from electricity affecting the installation.

"During the eclipse special care was taken to detect any disturbances, but nothing was recorded by the instruments."

Development of the Photographs.—Although provision was made for developing at Sfax trial photographs taken for adjustments before the eclipse, it was arranged to bring the eclipse photographs home and develop them at Greenwich, where the development could be carried on at greater leisure and under much more favourable conditions as regards temperature and water supply. The evening of the eclipse they were all taken out of their carriers and carefully packed in cardboard boxes. These were packed in tins, which were fastened by medical strapping so that they were air-tight. Although three weeks elapsed before any of the photographs were developed, no deterioration of any kind appears to have taken place.

The photographs were developed between September 21 and September 27 by Mr. Davidson and Mr. Melotte under Sir W. Christie's directions. In general the developer used was weak hydroquinone (one-third of the normal strength) and the development was continued for from 15 to 30 minutes. In a few cases pyro-soda or pyro-metol was used. The development was arranged so as to give a graduated series of photographs ranging from those showing the wisps of corona over the prominences to those giving all the extension it was possible to obtain. Previous to development Abney squares, with exposures adapted to the circumstances in each case, were printed on a number of the photographs.

II. *Photographs of the Corona.*

Four series of photographs were taken of the corona:—

(i) With the Thompson coronagraph on the scale of 4 inches to the sun's diameter;

(ii) With the object-glass of the astrographic telescope on the scale of 1¼ inches (32 mm.) to the sun's diameter;

(iii) With a Dallmeyer rapid rectilinear lens of 34 inches focus working at *f*/8 on the scale of 0·3 inch to the sun's diameter; and

(iv) With a Unar lens by Ross of 12 inches focus working at *f*/5 on the scale of 0·12 inch to the sun's diameter.

The photographs with the Thompson coronagraph would give the structural detail, particularly of the inner corona, while those with the astrographic object-glass should give a much greater extension in the coronal streamers. The photographs with the Dallmeyer are similar to those with the

astrographic object glass, but on a much smaller scale, while those with the Unar lens having a large field give the sky round the sun for the detection of a possible intra-mercurial planet.

Clouds round the sun unfortunately interfered with the coronal streamers, without, however, affecting the details in the inner corona, and the full advantage of the light grasping power of the astrographic telescope with the longer exposures was, to some extent, lost, the development having to be limited owing to the brightness of the sky background.

(1) *Photographs of the Corona on a Scale of* 4 *inches to the Sun's Diameter.*

(Taken by Sir William Christie.)

The instrument used was the Thompson photographic telescope with object-glass of 9 inches aperture and 8 feet 6 inches focal length, in combination with a concave telephoto lens by Dallmeyer of 4 inches aperture and 16 inches focus, fitted as a secondary magnifier, to give an image of the sun 4 inches in diameter, with a field for full pencils of 14 inches. The total length of the coronagraph was 12 feet, the equivalent focal length being 36 feet. The sun's light was reflected into the telescope by a 16-inch plane mirror with cœlostat mounting. The telescope was mounted on boxes loaded with 100-lb. shot, providing a firm stand, and was depressed about 5° in the azimuth 16° S. of W.

The adjustment of the cœlostat was made in the usual manner by observations of the declination of the sun with the attached theodolite. The clock was rated by observations of the sun's image on the ground glass of the camera. The telescope was focussed by means of the image of a gauze net in the plane of the plate reflected back from the plane mirror of the cœlostat,* in the same manner as in previous eclipses since 1896. It is of interest to note that the position of the focus was found to be the same as in the eclipses of 1900 and 1901.

The camera was furnished with eight plate holders to take 15×15-inch plates, or for the shorter exposures 12×10-inch plates in a carrier.

The following table indicates the plates taken, with the times of exposure reckoned from the beginning of the eclipse.

It was proposed to take eight photographs, but owing to the very bright group of prominences at the point of second contact, and a want of definition in the image on the ground glass, several seconds elapsed before it was realised that totality had begun. In consequence of this and a little difficulty with the plate holders, owing to the camera end being warped by

* 'Monthly Notices R. A. S.,' vol. 57, p. 105 ; 'Roy. Soc. Proc.,' vol. 64, p. 8 ; vol. 67, p. 397 ; vol. 69, p. 242.

No.	Exposure.			Plate.		Exposure of Abney squares.
	Beginning.	End.	Duration.			
	m. s.	m. s.	s.		in. in.	
1	0 26	0 31	5	Fine Grain	12 × 10	10 m. at 2 feet.
2	0 42	0 47	5	Sovereign	12 × 10	
3	0 58	1 9	11	Special Rapid	15 × 15	
4	1 22	1 42	20	Rocket	15 × 15	15 s. at 3 feet.
5	1 56	2 26	30	Special Rapid	15 × 15	30 s. at 3 feet.
6	2 43	2 53	10	Special Rapid	15 × 15	1 m. at 3 feet.
7	3 7	3 14	7	Sovereign	12 × 10	2 m. at 2 feet.

the sun's heat, only seven plates were exposed, the sun coming out just before the eighth plate could be exposed.

This plate was subsequently exposed for orientation about 20 minutes after totality, the aperture of the object-glass being reduced to 1 inch, and three instantaneous exposures being given at suitable intervals with the clock stopped.

It is of interest to note that totality occurred about 15 seconds before the time computed from the data in the Nautical Almanac, the local time being supplied by Commander Hyde from sextant observations.

Abney squares were printed after return to Greenwich on the Photographs 1, 4, 5, 6, 7 by exposure to a standard candle for the times and at the distances indicated in the above table.

The photographs were developed at Greenwich, normal hydroquinone diluted to one-third strength being used except for No. 4, for which pyro-soda similarly diluted was used. The time of development varied from 13 minutes for No. 2 to 30 minutes for No. 5, being carefully regulated by examination of the plates one by one as developed, so as to give a progressive series extending from the prominences and inner corona to the streamers.

The series of photographs shows very interesting detail in the inner corona associated with prominences, supporting the evidence for the connection between prominences and coronal structure shown in the photographs of 1898, 1900, and 1901, and exhibits the perspective of the coronal rays in a striking manner. The inner corona in this eclipse seems to be in a state of turmoil (all round the sun's limb), corresponding to the sunspot and prominence activity of the sun, oval rings and arched structures above the prominences being a special feature, whilst the streamers are relatively faint and generally distributed round the sun, without any indication of polar plumes or equatorial extension, the only feature suggesting polar plumes being at the sun's equator on the east side. Another interesting

D 2

feature is a dark ray strikingly shown as a vacuity in the coronal streamers. The very bright prominence on the east limb, extending over an arc of more than 30°, associated with oval rings and arches in the corona, is conspicuous on all the photographs from the beginning to the end of totality. As regards extension, coronal rays can be traced on No. 5 to a distance of fully 30′ from the sun's limb.

(2) *Photographs of the Corona on a Scale of 1¼ Inches to the Sun's Diameter.*

(Taken by Mr. Davidson.)

These photographs were taken with the object-glass of the astrographic equatorial. The aperture is 13·0 inches, or 0·33 m., and the focal length 135·1 inches, or 3·43 m., so that the scale is 1 mm. to 1′, and the diameter of the sun's image 32 mm., or 1·26 inches.

It was arranged to use this object-glass as a telescope fixed in an approximately horizontal position, in conjunction with a 16-inch cœlostat as in the case of the Thompson coronagraph.

A wooden tube was constructed at Greenwich to carry the object-glass, and to take plate-holders with 10-inch × 10-inch plates. The wooden tube, half an inch thick, was in three sections of equal length, provided with flanges by which the sections were screwed together. Two of the sections—the central one, and the one to which the object-glass was attached—were square, of 14 inches inside measurement. One of the sides of the third section (the top side as the tube was mounted for the observations) sloped downwards from a height of 14 inches to 12 inches in order to clear the lever which opened the dark slides. A wooden frame was fitted at the end of the tube to carry the dark slides, so that the 10 inch × 10 inch plates should be at the centre of the field. A mahogany block was fixed to the object-glass section of the tube, into which a steel ring of 13½ inches internal diameter and breadth 2¾ inches was let. The cell of the object-glass was attached to this ring by three adjusting screws in exactly the same way as it is attached to the tube of the astrographic equatorial.

The tilt of the object-glass and of the plate were adjusted by means of a small collimating telescope. The focus was carefully determined by photographs of stars taken at Sfax, and the necessary adjustment made by moving the object-glass. An examination of the eclipse photographs shows that the focal adjustment was remarkably good.

The adjustments of the cœlostat were made by means of the attached theodolite, and the clock was rated by means of the image on the ground glass.

The following table shows the plates taken, with the times of exposure:—

No.	Exposure. Beginning.	Exposure. End.	Exposure. Duration.	Plate.	Exposure of Abney squares.
	m. s.	m. s.	s.		
1	0 23	0 25	2	Fine Grain	
2	0 43	0 48	5	Fine Grain	4 m. at 3 feet
3	1 6	1 16	10	Ordinary	
4	1 53	1 58	5	Sovereign	
5	2 2	2 22	20	Sovereign	
6	2 40	2 45	5	Ordinary	4 m. at 3 feet
7	3 3	3 8	5	Fine Grain	90 s. at 2 feet
8	3 26	3 28	2	Fine Grain	

While the last exposure was being made totality ended. The inner corona is, however, well shown, except near the point of third contact.

The photographs Nos. 1, 2, 3, 4, 5, and 8 were developed with hydroquinone of one third of the normal strength for times varying from 10 m. to 40 m. No. 6 with pyro-soda, and No. 7 with pyro-metol, similarly diluted. The greatest extension is shown on No. 5, which shows the corona to a distance of about 70′ from the sun's limb. With a sky free from haze and cloud, much greater extension would doubtless have been obtained, as the development had to be stopped when the sky came up on the plates. The photographs with shorter exposures on slower plates show the structure of the corona in beautiful detail, and supplement and confirm those taken with the Thompson coronagraph.

(3) and (4) *Small Scale Photographs.* (Taken by Professor Sampson.)

The double camera was used exactly as at Ovar and in Sumatra, mounted equatorially on the stand of the Dallmeyer photoheliograph. The Dallmeyer rapid rectilinear lens is of 4 inches aperture and 34 inches focus, and the Unar lens, by Ross, of 2·4 inches aperture, and 12 inches focus. Seven plate-holders, each taking a pair of 16 cm. × 16 cm. plates, were used during totality. The exposures, to avoid the possibility of shake, were made by holding a cover in front of the object-glasses. The exposures were as follows:—

No.	Exposure.			Plate.	Exposure of Abney squares.	
	Beginning.	End.	Duration.		Dallmeyer.	Unar.
	m. s.	m. s.	s.			
1	0 23	0 25	2	Fine Grain	2 m. at 3 feet	4 m. at 3 feet
2	0 43	0 48	5	Fine Grain	4 m. at 3 feet	
3	1 6	1 16	10	Ordinary		45 s. at 3 feet
4	1 34	1 44	10	Sovereign		
5	2 2	2 22	20	Sovereign		
6	2 40	2 45	5	Ordinary		
7	3 3	3 8	5	Fine Grain	4 m. at 3 feet	

The development was usually with weak hydroquinone as in the case of the photographs taken with the Thompson and astrographic telescopes.

Both series of photographs show the coronal streamers well defined and to a considerable distance, the corona being shown on the Dallmeyer photograph No. 3 and on the Unar photographs Nos. 3, 4, and 5 to a distance of nearly 90′ from the sun's limb. These photographs have unquestionably suffered owing to the clouds. Nos. 3 and 5, with the Unar lens, have been examined, and the stars over the field of 15° radius from the sun are shown as follows:—

B.D., 10°, 2166, 4·1 m.	Shown.
9°, 2374, 6·2 m.	Not shown.
11°, 2283, 5·3 m.	Shown.
16°, 2234, 3·3 m.	Shown.
11°, 2384, 4·1 m.	Not shown.*
6°, 2437, 4·3 m.	Not shown.*
B.D., 8°, 2455, 5·2 m.	Doubtful.
6°, 2384, 5·2 m.	Shown.
4°, 2407, 4·5 m.	Not shown.
Regulus, 1 m.	Shown.
Mercury	Doubtful.

No other object is shown on the plates.

(3) *Spectrographs of the Corona and Chromosphere.* (Taken by Mr. Dyson.)

Major Hills again kindly lent the two photographic spectroscopes used by him at the Indian eclipse and by Mr. Dyson at Ovar and in Sumatra. The details of the spectroscopes are shown in the following table:—

	Spectroscope No. 1.	Spectroscope No. 2.
Objective	Cooke, achromatic, 4½ in. aperture, 6 ft. 2¼ in. focus	Single quartz lens, 5 in. aperture, 4 ft. 7 in. focus
Collimator and camera lenses	Single quartz lens, 2½ in. aperture, 30 in. focus	Single quartz lens, 3 in. aperture, 33½ in. focus
Prisms	Two dense flint of 60°, 4½ in. base, 2¼ in. height	Four double quartz of 60° (each of two half prisms of right- and left-handed quartz), 3½ in. base, 2¾ in. height
Prisms at minimum deviation	H_γ (λ 4340)	λ 3600
Slit	1½ in. by 0·0015 in............	2 in. by 0·0012 in.

* Near edge of plate.

The width of the slit was adjusted by means of the diffraction images according to the method given by Mr. Newall.

In order to get a greater length of spectrum in focus the plate-holders were made so as to carry two plates $3\frac{1}{4}$ inches wide, inclined at a suitable angle, instead of each carrying one of $6\frac{1}{2}$ inches. The length of the spectra are as follows:—

Flint spectroscope, green end, 38 mm. from D_3 (λ 5876) to H_β (λ 4861).

Flint spectroscope, blue end, 59 mm. from λ 4500 to H (λ 3968).

Quartz spectroscope, blue end, 55 mm. from the helium line at λ 4471 to the titanium line at λ 3685. Nothing was shown on the plate in the extreme ultra-violet.

The spectroscopes were mounted horizontally and supplied with light by a heliostat with a 12-inch mirror. The two spectroscopes were adjusted so that the slits were nearly tangential to the sun's limb. The following diagram shows the position of the sun's image on the slit as nearly as it can be inferred from the spectrographs. The position of the prominence has been drawn on the diagram from one of the coronagraphs. It happened that the second contact coincided within 1° with the point when the sun's image reflected by the heliostat could be made to touch the slit, and the spectroscopes were adjusted for the slits to be as nearly tangential as possible. The

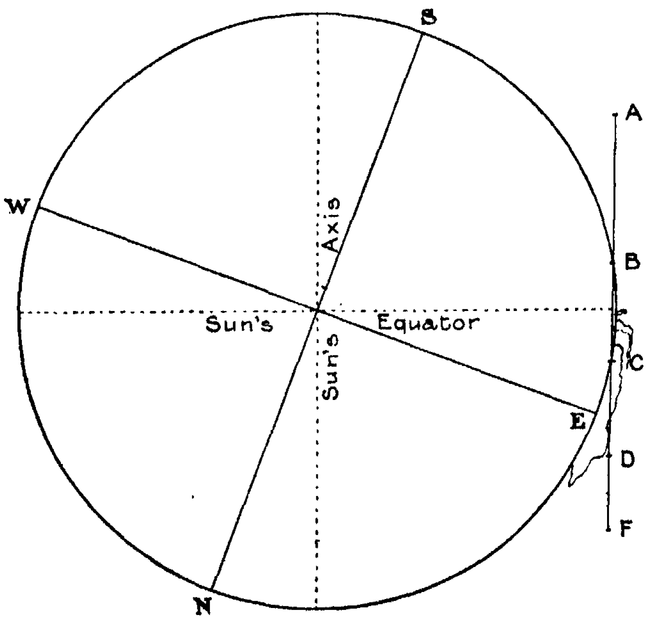


Diagram showing the position of the slit with reference to the sun's image and the extent of the corona obtained with the spectrograph.

"flash" spectra were unsuccessful, but extremely interesting spectrographs of the corona and the large prominence were obtained. The exposure was approximately 3 minutes, lasting from about 20 seconds after the beginning to 10 seconds from the end of totality. The plates used were Barnet orthochromatic in the green and Rocket in the blue and violet.

In the above diagram, which is drawn to scale, the line A B C D F gives the breadth of the spectra obtained. The continuous spectrum is shown corresponding to the lines A B and C F, but is strongest in the part A B. Chromospheric lines are shown in the part corresponding to C D, and end sharply. The part corresponding to B C is blank, except that the bright corona lines at 5303 and 4231 stretch right across it. No chromospheric lines are seen in the part A B.

The continuous spectrum is shown to a distance of 3′·3 from the sun's limb. It is shown on the different photographs from D_3 (λ 5876) to λ 3550.

In the bright-line spectrum two new lines are clearly shown at λ 5536 and λ 5117. These are shown where the corona spectrum is strongest, and where there are no prominence lines. Their position is fixed relatively to the green coronium line at 5303. The lines 4231, 3987, and 3801 are also strongly shown, while lines at 4361, 4086, and 3643 are faintly shown. The lines at 5536 and 5117 are probably the lines which have been referred to by previous observers, as seen in the neighbourhood of the green line. The green line 5303 is very strongly shown.

As the green coronal line stretches through the part of the spectrum where the prominence lines are shown, it is possible to determine its wave-length with reference to them. Unfortunately they are somewhat distant. The following table gives the measures and the computed wave-lengths of the

Measure.	Wave-length computed.	Tabular wave-length.	T—O.	Line.
294·530	5875·87	5·87	0·00	D_3, helium
264·838	5535·80	—	—	New corona line
240·391	5303·10	—	—	Green corona line
226·228	5183·79	3·79	0·00	b_1, Mg
224·870	5172·88	2·86	+0·02	b_2, Mg
224·444	5169·48	9·22	+0·26	Fe, enhanced line
224·200	5167·53	7·50	+0·03	b_4, Mg
217·833	5117·69	—	—	New corona line
204·456	5018·85	8·63	+0·21	Fe, enhanced line
204·011	5015·69	5·73	−0·04	Parhelium
190·611	4924·08	4·11	+0·08	Fe, enhanced line
190·290	4921·92	2·10	+0·18	Parhelium
180·881	4861·53	1·53	0·00	H_β
155·629	4713·29	3·25	+0·04	Helium
150·584	4685·86			

green line and the two new lines, the lines D_3 (λ 5876), b_1 (5184) and H_β (4861) being used to determine the constants of Hartmann's formula. Judging from photographs taken in Sumatra and reduced with these three lines as standards, the correction required by the formula near λ 5303 cannot exceed 0·2. The wave-lengths are provisional to this extent, and measures will be made to determine, if possible the amount of this correction.

The wave-length of the blue coronal line is found to be 4231·1. The wave-lengths of the other lines have not yet been determined.

The possibility of a party from the Royal Observatory occupying a station in Labrador was considered at first, but afterwards given up as impracticable. In April, however, Mr. Maunder received an invitation to join the party sent out by the Canadian Government to Hamilton Inlet at the head of Lake Melville. In this way the possibility arose of obtaining comparable large- and small-scale photographs of the corona taken at a considerable interval of time apart. Mr. Maunder took with him the Dallmeyer coronagraph, a 4-inch lens which, in combination with a 3-inch concave telephoto lens, formed a telescope of 21 feet equivalent focal length, and gave an image of the sun of 2½ inches. He also had the Abney lens belonging to the Royal Astronomical Society, a lens exactly similar to the Dallmeyer lens used by Professor Sampson at Sfax. Unfortunately it was completely overcast on the day of the eclipse, and no observations could be made.

Report of the Expedition to Pineda de la Sierra, Spain.

By J. EVERSHED, F.R.A.S.

(Received October 31, 1905.)

In the following paragraphs I summarise the principal objects for which this expedition was undertaken :—

(1) To obtain large scale images of the flash spectrum with a prismatic camera of great focal length, for the purpose of studying the actual forms assumed by the different radiating gases, and to obtain evidence regarding the probably eruptive nature of the gases giving the enhanced lines.

(2) To obtain ultra-violet spectra of the corona with prismatic cameras of glass and of quartz, using the flash-spectrum arcs as reference lines for determining wave-lengths.

(3) To study the corona spectrum in the D—F region with high dispersion, visually and by means of photographs, to ascertain the character of the continuous spectrum and of the radiation at λ 5303.

Instruments Available.

Special provision was made out of the Government Grant Fund for the construction of the essential parts of the large prismatic camera, and a concave mirror of 15 inches aperture and 29 feet focus, and a dense flint prism of 40° angle transmitting a beam of 7 inches diameter, were made for the expedition by Sir Howard Grubb.

A 16-inch cœlostat was also provided from the same fund, and a large revolving plate-carrier for exposing in rapid succession a series of 15 plates, each 15 inches × 7 inches.

For the ultra-violet work on the corona I had a glass prismatic camera of 2 inches aperture and 46 inches focal length, having two 60° prisms of special glass, transparent as far as λ 3300. The lens by Hilger is very perfectly achromatised between H_β and λ 3300.

This instrument had already seen service in India in 1898 and Algeria in 1900. It was supplied with light from a 4-inch speculum metal flat lent me by Mr. Maw, and which I attached to the upper end of the cœlostat axis parallel with the 16-inch silver-on-glass mirror.

Supplementary to this very efficient spectrograph I had a quartz prismatic camera, mounted on an equatorial telescope, and receiving light direct from the sun. It contains two double prisms of right and left-handed quartz, and a single lens of 1-inch aperture and 36 inches focus for D.

A direct-vision slit spectrograph was put together while in camp. It was built up from parts of other apparatus, and consisted of image lens, slit and collimator, three powerful direct-vision prisms of 1-inch effective aperture, and a single camera lens of 47 inches focus.

For visual work on the corona spectrum I took out my complete outfit for prominence observations, consisting of a $3\frac{1}{4}$-inch equatorial telescope and driving clock, and a high-dispersion 3-prism spectroscope.

On the Selection of the Observing Station.

As the main purpose of the expedition was to secure large scale photographs of the flash spectrum arcs, first-rate atmospheric conditions as regards definition were considered essential. Large apertures and great resolving power are of no avail unless the "seeing" is of really good quality, and it was to be feared that in the plains near the east coast of Spain, or even in the immediate neighbourhood of the town of Burgos, the chances of really good definition would be very poor, owing to the high mid-day temperatures. Another important point was to select a spot at a little distance from the central line of eclipse, where the internal contacts should occur in the sun-spot zones, as I wished, if possible, to photograph the spectrum of a metallic eruption as displayed in the lower chromosphere.

My choice of a suitable place was simplified by the circumstance that in the immediate neighbourhood of Burgos there is a mountain region accessible by railway. This line, built by the Sierra Company of London and Burgos, runs from Villafria, near Burgos, in a south-easterly direction, into the heart of the mountains. Ascending about 1000 feet above the plain it penetrates the mineral region of the Sierra Demanda range, and is intended for transporting the coal and iron and other minerals from the properties of the Sierra Company. The general direction followed by the line happens to coincide roughly with the shadow track of the eclipse, and at exactly the distance from the central line that I wished to be located. It was therefore only necessary to fix on a suitable locality at practically any point on the line at a sufficient elevation above the plain.

Yet another consideration which determined me in the final selection was the character of the ground as regards vegetation. It seemed to me a mistake to erect a large horizontal telescope on bare earth or upon stony or rocky ground; quite apart from the question of dust, there is the more serious objection that the air is apt to be disturbed by convection currents by contact with the ground, particularly when the latter has been heated by the sun. Where there is thick vegetation, on the other hand, the sun's rays have little heating effect, the leaves of the plants in performing their

functions directly absorbing the solar energy. It seemed best therefore to find a spot where there was plenty of herbage, and to raise the mirrors and prism as high as possible above it.

My friend, Mr. J. H. White, of Burgos, who is resident manager of the Sierra Company, had given me much useful information beforehand with regard to the railway and the character of the country, and it was owing to the indispensable aid he rendered my expedition on arrival in Spain that we were able, without any loss of time, to select an almost ideal position near the village of Pineda de la Sierra, a point on the line about 30 miles from Burgos.

This was on a heath 4000 feet above sea-level, and nearly surrounded by mountains rising about 2000 feet higher. It was north of the central line of eclipse, the internal contacts being in solar latitude $+ 5°$ and $+ 15°$, and the duration of totality 220 seconds.

In choosing an open heath on the side of a mountain I seem to have been especially fortunate in securing extremely good seeing conditions as well as a cool climate. The vegetation consisted of common heather in full flower, interspersed with broom and juniper bushes. Higher up on the hills beech woods and forests of broom and stunted oak covered the ground, and even on the highest slopes of the mountain to the south of the camp, which I ascended, the bare rock was very little exposed, except on the northern precipitous face, being mostly covered with a large species of heath.

Whether this beautiful clothing of vegetation had anything to do with the homogeneous state of the air or not it is impossible to say, but it may be worth while recording the facts. I had many misgivings at first as to the probable effect of the high mountain slopes surrounding the camp, which was very much shut in by mountains, but experience showed these to be ill-founded, the definition being the best and most uniform I have ever experienced for the sun.

The expedition travelled to Spain in two separate parties. I left England on July 29, accompanied by Mr. R. C. Slater, M.A., who had kindly volunteered to act as my assistant, and to whom my best thanks are due for his very efficient aid in adjusting the instruments and in many other ways. We travelled by sea to Bordeaux, and on the steamer had the pleasure of falling in with the other official expedition to Spain under Professor Fowler, a most opportune meeting, as we had many matters to discuss and arrange with regard to our spectroscopic observations.

The second contingent left for Spain two weeks later. It consisted of my brother, Mr. H. Evershed, who had helped me at a former eclipse, and the Rev. C. D. P. Davies, M.A., who was bringing out some instruments

of his own design which he intended setting up within the enclosure of my camp.

Mr. Slater and myself reached Burgos on August 2, and the next day Mr. White arranged for an engine and brake van to take us on a prospecting expedition as far as Barbadillo de Hereros, a village about 40 miles from Burgos. It was on the return journey the following day that I decided upon the heath near Pineda as the most suitable site for the camp.

After some delay in getting the instruments transported from Bilbao,* the port to which they had been shipped, to Pineda, the first party got to work on August 12.

The Sierra Company had kindly placed at my disposal a number of tents which, with bedding taken from one of their houses, afforded us ample sleeping accommodation as well as store room for the instruments and boxes. A barbed-wire fence put up round our "claim" secured us against possible incursions of wild boar at night from the great beech wood which clothed the mountain to the south of us. From human beings we had nothing to fear. Mountaineers are usually honest folk, except when they are brigands, and the people of Pineda were entirely friendly, and much too busy with their most interesting harvesting operations to pay much attention to us.

In all the initial work of erecting piers, huts, etc., we had the invaluable assistance of the engineer of the Sierra Company, Mr. C. Ellis Bevan, who obtained for us everything we needed in the way of materials, and stayed several days in the camp, superintending the work of the masons and carpenters and helping us in many other ways. I have much pleasure in expressing here my appreciation of the very satisfactory way in which all this preliminary work was carried out.

On August 18 the second contingent of the expedition arrived in camp. Mr. Davies was bringing out a reflecting coronagraph of 74 inches focus, and a reflecting prismatic camera of about 60 inches focus, both of which instruments he intended to work from a cœlostat which he had ingeniously contrived out of an old equatorial stand. This was fitted with a driving clock, to which he had attached special gearing for reducing the rate to half speed. My brother was to assist me with the instruments on the day of the eclipse, he also had charge of the time determination and finding the correct position of the camp, and he soon got to work with the sextant and artificial horizon.

* Through the good offices of Mr. W. Henry Hodgson, of Bilbao, I was able to forward the 22 cases of instruments by fast train at the ordinary goods rate and to stop the train at the Sierra Company's siding for transference to their trucks. These concessions on the part of the Norté Railway were not obtained, however, without much tedious waiting and it was nearly a week before the instruments arrived at Villafria Siding.

The exact distance and bearing of San Millan, a mountain to the north-east of the camp, he ascertained by measuring a 500-feet base-line and observing the bearings of the cairn on the top from each end of his base. This gave us a determination of latitude and longitude, depending on the position of this point, the co-ordinates of which had been communicated to me from the Madrid Observatory.* The latitude so obtained was in good agreement with the observed latitude by meridian observations of the sun. The mean results are as follows:—

North latitude..............	42° 11′ 16″
West longitude	0 h. 13 m. 4 s.
Altitude above sea-level...	3986 feet

The determination of longitude was of some importance, as we had to depend entirely on observations for finding G.M.T., and it was desirable to know this within a second or two on the day of the eclipse. The longitude from San Millan differed by about 10 seconds of time from that shown on the maps I was able to consult.

We had brilliant weather and an almost entire absence of wind during the 18 days in camp, which greatly facilitated the work of erection and adjustment of the instruments. I had been assured by Mr. White that our chances of failure from cloud were no worse in the mountains than at Burgos, and our daily experience quite confirmed this.

As soon as I had the equatorial telescope mounted, daily observations were made of the prominences, and, with the exception of August 29, which was overcast and rainy, an unbroken series of observations was secured from August 14 to August 31 inclusive.

I have great pleasure in acknowledging here our indebtedness to Mr. White, not only for giving every facility which the railway afforded free of all charge, but for practically devoting himself to the interests of the expedition throughout our stay in Spain. Upon our arrival at Burgos, Mr. and Mrs. White most kindly placed their flat at our disposal, and we also enjoyed their generous hospitality on our return from Pineda after the eclipse. Our acknowledgments are also due to Mr. Williams, the courteous managing director of the Sierra Company in London.

Adjustment of the Instruments.

The cœlostat was adjusted very easily by means of the attached theodolite, using stars at considerable hour angles east and west of the meridian for azimuth. In order, however, to adjust the other instruments in relation

* I am also indebted to Mr. Hodgson and to Don José Esteban Clavillar, a surveying engineer, for obtaining this information for me.

to it, I found it convenient to put the axis out of adjustment in azimuth by unscrewing one of the two adjusting screws and pushing the lower end of the axis bodily towards the east a few degrees. It was then possible to reflect a horizontal beam of sunlight in the required azimuth, notwithstanding the greater declination of the sun at dates previous to the eclipse. The screw on the west side being left untouched, it was only necessary as the days went by to slowly screw up the east screw until the base again came into contact with the west screw on the day of the eclipse, the instrument being then again in correct adjustment..

The driving clock was mounted on a wooden frame, supported at one end by the cœlostat pier and at the other by two posts driven into the ground about a foot from the south face of the pier. The weights were hung underneath, direct on the winding barrel, the height of the clock above the ground giving a sufficient drop to keep it running about 20 minutes.

Mr. Slater had charge of the clock and regulated it to a nicety by erecting a temporary horizontal telescope, receiving light from the cœlostat; this gave an enlarged image of the sun on a paper screen, ruled with lines at right angles to the diurnal motion. The relative drift of a sun-spot could in this way be observed with great ease even at a considerable distance from the screen, and the clock adjusted accordingly.

The Reflecting Prismatic Camera.

The general arrangement of the apparatus is shown in the accompanying plan-diagram, excepting that the 15-inch concave mirror was too far away from the other parts to be conveniently shown on any reasonable scale.

The azimuth of a horizontal beam of sunlight reflected west by the cœlostat was 12° 24′ south of west on the day of the eclipse, but the deviation of the prism being about 31° for the line H_γ, which it was intended should fall near the centre of the plate, the principal axis of the instrument was arranged in a direction 18° north of west and south of east, and the mirror, prism, shutter and plate-carrier were placed in line in this azimuth.

The plate-carrier, or exposing machine, was mounted on a wall of masonry about 2 feet high and 6 feet long, erected a few feet to the south-west of the cœlostat pier. This apparatus was designed to make 15 exposures in rapid succession, seven at second contact, one about mid-eclipse, and seven at third contact. The plates are held in compartments on the circumference of a steel drum, 4 feet in diameter, mounted on bearings, and enclosed within a large box of wood, having a sheet-iron light-tight cover. There is a rectangular opening in front of the box the same size as the plates, which are brought successively opposite to it by rotating the drum.

A crank handle outside the box is connected by gearing with the drum, so that one rotation of the handle moves the drum exactly one-sixteenth of a revolution. There are 16 compartments, one containing a plate of clear glass, used as a focussing screen. Behind this plate is a light-tight box, containing a right-angle prism which reflects the light through a tube at the side of the box into a small telescope attached to the outer case.

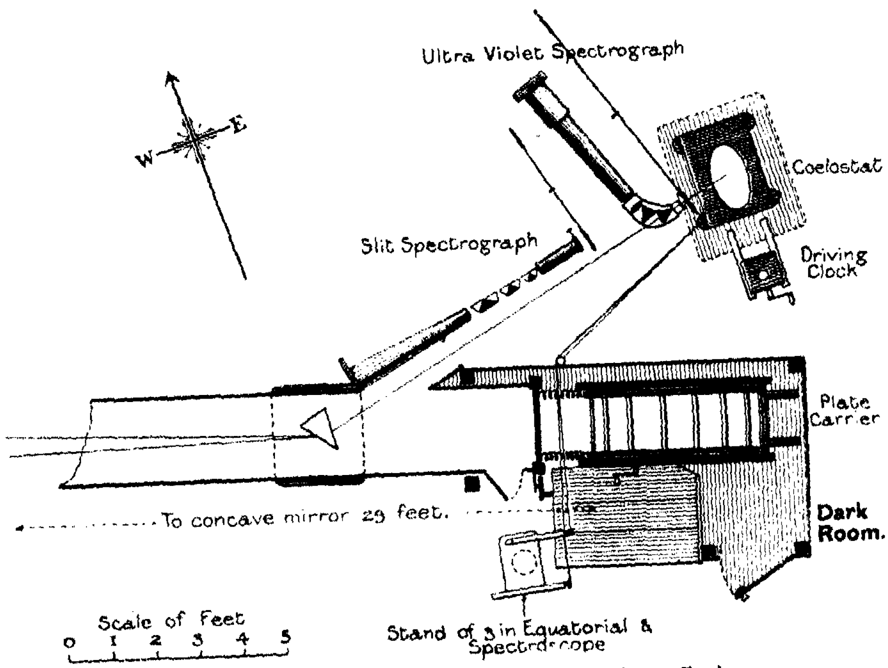


FIG. 1.—Plan of Observatory, Pineda de la Sierra, Spain.

To effect the focussing of the image on the plates, the entire apparatus was made to slide on Λ-shaped steel rails fixed on the top of the masonry. It was worked to-and-fro by means of rackwork and a pinion wheel, the latter having a handle attached outside the case.

In front of the plate-carrier and connected to it by extensible photographic bellows the large focal plane shutter was mounted on the same wall of masonry. It was worked by hand up and down by means of a steel wire rope passing over pulleys, the end of the rope being wound round a drum having a handle conveniently placed near the handle actuating the plates. With these two handles, therefore, one could change the plates and make the exposures alternately, using the right and left hands.

The whole of this apparatus was protected from the weather by a hut of matched boards built over it. This was extended at the S.E. corner to form a small dark room to facilitate getting the plates in and out of the machine.

Four feet in front of the shutter a separate pier was built 1 metre high and ½ metre square to support the 40° prism. On the top of the pier a stout mahogany board with cross pieces screwed underneath was firmly secured with cement; and a turned steel pin, 1 inch in diameter, fixed to the board on the north side formed a vertical axis for the rotation of the prism in the plane of dispersion. Centred upon this pin was a heavy brass segment with tangent screw attached, the bearings of the worm and handle being screwed to the base board. Upon the brass segment another thick mahogany board was fastened, and the carefully planed upper surface of this supported the prism, which was secured from lateral displacement by small angle pieces at the corners of the prism.

A light tube, 2 feet square in section, made of Willesden paper, nailed to a frame of wood, enclosed the space between the prism and the shutter, and extended beyond the prism a few feet in the direction of the concave mirror. A branch tube on the north side admitted light to the prism from the cœlostat. The paper covering of this tube was not fastened along the top, which could, therefore, be opened at any point or all along, in order to give free ventilation and prevent the possibility of non-homogeneity in the air inside the tube. On the day of eclipse the tube was left open for half its length until a few minutes before totality.

The concave mirror was mounted on a pier 23 feet distant from the prism. The cell was attached to a heavy cast-iron support, having a vertical face from which projected three equi-distant strong steel bolts, screwed at the ends, which passed through the corresponding holes in the mirror cell, holding the latter in a vertical position. Between the back of the cell and the casting each bolt passed through a stout spiral spring 3 inches in length. The springs were in compression when the cell was in position with fly nuts on the ends of the bolts, and the mirror could be nicely adjusted at three points by turning these nuts by hand. It was necessary to tilt the mirror about 40′ from the vertical in order that the return rays should clear the top of the prism and fall unobstructed upon the plates.

It was thought advisable to have no tube between the mirror and prism excepting the short length of paper tube already mentioned. To keep out light two oblong tents were erected, one over the mirror and the other between it and the prism, and in order to cover the whole space of 23 feet the tents were pitched a few feet apart, and the intervening space covered with a piece of canvas. The canvas at each end of the two tents could be tied back so as to admit of a good draught of air through both to equalise the temperature inside and out, and it was only necessary a few minutes before totality to let down the canvas at the end behind the mirror to exclude light from outside.

Method of Focussing.—The focal length of the concave mirror was carefully measured before leaving England with a 50-foot steel tape, using the same cœlostat and focussing on sun-spots and star images. In order to focus the prismatic camera approximately the plate-carrier was moved in its ways by means of the rackwork and pinion until the front surface of the front plate was precisely the same focal distance, measured with the same tape, from the concave mirror. But as some slight alteration of focus might occur from the action of the prism, it was intended to observe the edge of the cusp spectrum near the H_β line, about five minutes before totality, in the small telescope attached to the side of the plate-carrier.

Owing to clouds, however, it was found better to observe a strip of spectrum in the green region from the front of the plate and without using any lenses. This part of the glass plate which formed the focussing screen, was partly covered with a strip of white paper gummed to it, and the cusp spectrum, though partly obscured by thin cloud, could be clearly seen upon it and focussed fairly well.

The Ultra-Violet Prismatic Camera.

This instrument was fixed in an inclined position on the top of a packing-case near to the cœlostat. It received light from the 4-inch speculum flat attached to the upper end of the cœlostat axis. The position of the cusps at the internal contacts with respect to the refracting edges of the prisms, necessitated an inclination of the camera body of about 22° to the horizontal, in order to have the flash spectrum arcs at both contacts equally inclined to the direction of dispersion.

Nine exposures were to be made by racking a series of plates along a long dark box fixed at right angles to the camera body. At least four of the exposures were to be out of totality, in order to get good images of the cusp and flash spectra, which it was hoped would provide an accurate scale for estimating the wave-lengths of the ultra-violet coronal rings. This method had been found trustworthy in some photographs obtained in 1898, showing a faint ring at λ 3388. A light metal disc covering the aperture to the prisms and attached to a long wooden rod hinged at the centre, formed a simple and convenient exposing shutter.

The focus of this instrument was very carefully determined before leaving England by attaching it to a Newtonian reflector of about three times its focal length, and using the reflector as a collimator, placing a slit in its focus and directing it to the sky. A series of plates of the Fraunhofer spectrum was obtained and the exact distance between the back of the camera lens and the front of the plate, when at the sharpest focus, was measured with a metal rod.

On setting up the instrument for the eclipse it was only necessary to make certain that this distance was preserved within 0·005 inch.

The Slit Spectrograph.

Light being available from the upper part of the 16-inch cœlostat mirror, it was possible to arrange this apparatus between the cœlostat and the large prism of the reflecting prismatic camera, the optical axis being horizontal and slightly above and to one side of the beam of light entering the large prism. The combined action of the three compound prisms caused a deviation of a few degrees of the green part of the spectrum which it was desired to photograph, consequently the camera end could be arranged quite clear of the large prism and outside the paper branch tube, as shown in the diagram.

The focus was satisfactorily found by projecting the sun's image on the slit and focussing the Fraunhofer lines on the film side of an old negative, observing with a lens through the back of the plate.

Mr. Slater had charge of this instrument and was to make two long exposures during totality with the slit tangent to the sun's limb near the point of third contact.

The Quartz Prismatic Camera.

This was clamped to the 3-inch equatorial telescope in such a way as not to disturb its balance. The prisms received light direct from the sun and it was proposed to make one exposure only during totality.

This camera was approximately focussed by using a 9-inch mirror as collimator and photographing the Fraunhofer spectrum. Owing, however, to the non-achromatic single quartz lens used, the necessary inclination of the plates made it impossible to expect a perfect focus for complete coronal rings, and it was intended to use this camera merely to obtain confirmatory evidence of faint coronal rings which might be indicated in plates obtained with the perfectly achromatised glass prismatic camera.

Observations made on the Day of Eclipse.

The morning of August 30 broke perfectly cloudless after dull rainy weather the previous day. Our hopes of a successful result, which had been considerably depressed by the rain, rose far above the miserable mean of uncertainty. There were indeed pessimists among us who thought it possible that clouds might appear later.

At 10 A.M. the position angles of the prominences were determined and a drawing made of a fine group on the east limb extending from solar latitude

+9 to +33.* I give here a reproduction of this drawing, in which the position angles, and points of contact at Pineda, are shown.† The solar spectroscope was afterwards turned in position angle and the slit set tangent at 305°, the place of first contact. The chromosphere being beautifully distinct it was hoped to observe the approach of the moon's limb some 20 seconds before the contact. In the meantime, however, a considerable amount of cloud *had* appeared in the sky and at the time of contact the sun was hidden. *It reappeared after the eclipse had made fair progress and observations were continued at intervals.*

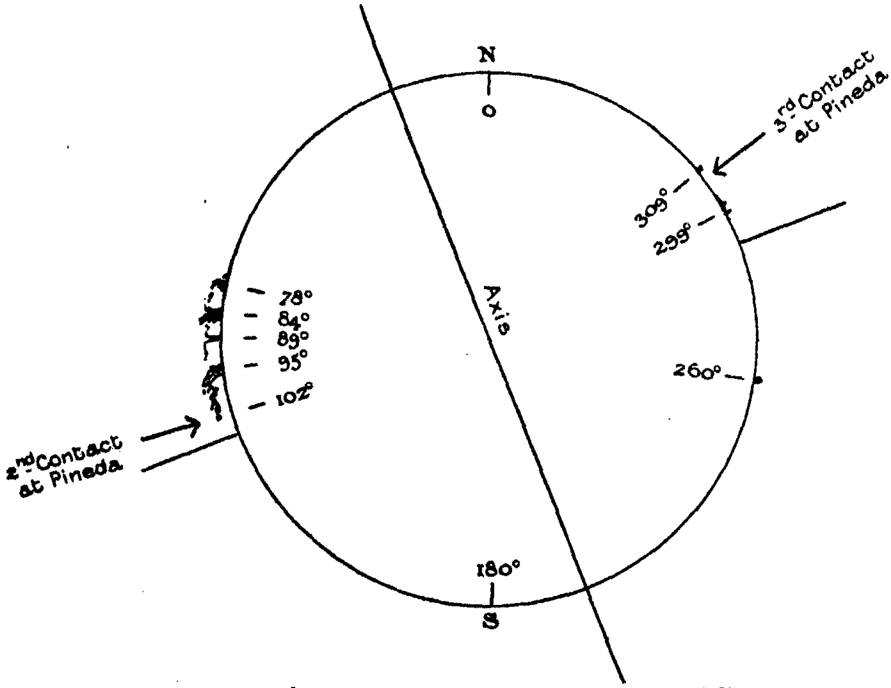


FIG. 2.—Prominences observed at 10 A.M., August 30, 1905, at Eclipse Camp, Pineda de la Sierra.

When about three-fourths of the sun was eclipsed the spectrum of a large spot near the east limb was critically examined to see whether the diminished

* This group of prominences had already been observed on the N.W. limb on August 16 and 17, when it occupied the region +10 to +31, and it was again seen on the N.W. limb on September 13, very little changed in size or general character.

† It will be seen from fig. 2 that the positions of the contacts at my station were extremely favourable for getting the spectrum of the base of the prominences during the moments of visibility of the flash spectrum, and had the sky been clear at the right moment I should have secured the spectrum of a small but brilliantly "metallic" prominence, which Professor Fowler tells me he observed at P.A. 306°.

sky illumination had any appreciable effect. The spot band appeared unusually dark, so that it was difficult to trace the Fraunhofer lines across the nucleus, but this may have been partly due to the intrinsic darkness of this particular spot.

As the sky illumination gradually diminished with the progress of the eclipse, the prominences on the east limb became more and more vivid in the H_a line, and it would have been most interesting to observe them up to the last moment before totality, but at 12.45 P.M. it was necessary to prepare for the photographic operations.

At this time the crescent sun was shining in a clear blue space between rather heavy clouds, and in a few minutes the general illumination began to assume that peculiar blueish tint only seen at total eclipses. There was no wind and the clouds appeared almost stationary.

The dim light of the thin crescent seemed already giving out, but there were yet 10 minutes of anxious waiting, and the clouds getting slowly nearer. Before 1 o'clock thin cloud had already covered what was left of the sun. Still one could see faint patches of light on the mountain sides marking the favoured spots, and there was yet a possibility that one of these might drift over the camp in time.

At 1 o'clock Mr. White, who had come up from Burgos to assist me with the visual observations, took up his station near the equatorial ready to note down any observations I might make during the eclipse.

Mr. Slater wound the cœlostat clock, let down the cover of the paper tube over the large prism, and then stood by to slightly move the prism tangent screw at my direction. My brother also went to his place by the ultra-violet prismatic camera, ready to begin his series of exposures at a signal from me.

At 1.5, looking in at the door in front of the shutter, I could see the spectrum faintly upon the glass focussing plate and by slight movements of the prism and of the cœlostat,* I adjusted it exactly to the correct position on the plate and slightly altered the focus.

The next operation was to estimate the width of the cusp spectrum by comparing it with the strip of white paper gummed to the glass plate. This strip was cut out the exact width which the spectrum should have when only 30 seconds remained before totality, and it was my intention to start a stop-watch at this instant and to begin the exposures after the lapse of 15 seconds.

This method I think would have been quite successful had it not been for

* I was able to control the cœlostat very conveniently by means of an endless cord which actuated the slow motion. This passed through a hole in the side of the hut, and round a pulley attached to the free end of a movable arm. A weight pulling on the arm kept the cord at a constant tension.

the clouds, which, partly obscuring the horns of the crescent, made the edges of the spectrum indistinct. As it was I estimate that I started the watch 25 seconds too soon. Having started it I called out "Are you ready," and then about 20 seconds later, "Now." I then started the exposing machine and the focal plane shutter, and my brother simultaneously began his exposures.

I glanced at the sky, and knew from the size of the cusp still visible that I had begun too soon. To allow for this I continued the exposures very deliberately until the increasing darkness warned me of the approach of totality, so that during the critical moments plates were exposed in rapid succession. I exposed in all eight plates, and then turned to examine the corona spectrum in the solar spectroscope and expose the quartz prismatic camera. The hopelessness of the situation then became evident. Thick cloud covered the place of the sun. I took up a pair of binoculars, and, after a little waiting, just glimpsed a faint ring of light for one moment. It was the inner corona showing through the clouds.

At the approach of third contact I abandoned the instruments and simply watched the wonderful effects on the sky and landscape. There were several deep violet rifts and patches of clear sky, in one of which Venus shone resplendent. The limit of the shadow was clearly visible as a long line of light low down in the north-west, and this quickly spread upwards, and the great blue shadow rushed over the mountains and disappeared to the east of us. About a quarter of an hour later the crescent was again visible.

It was small consolation to hear by telephone that at Arlanzon village, half-way between Pineda and Burgos, a perfect view was had of the whole eclipse! Mr. Bevan, the engineer of the railway, studied the effect of it on his fowls!

Results.

The first four exposures with the large prismatic camera yielded well-defined images of the cusp spectrum. Although much obscured by cloud, the Fraunhofer lines are clearly shown, and the hydrogen lines β, γ, δ, and the lines H and K are just visible as bright lines along the edges of the spectrum. Measures of the chord of the arcs in the stronger dark lines give the following times of exposure :—

Exposure No.	Interval before Second Contact.
1	43 seconds.
2	37 „
3	30 „
4	19 „
5	No trace of image

At the time of the first exposure the centre of the cusp had a width of 17″, corresponding to a linear width on the plate of 0·029 inch. The spectrum arcs are consequently not well resolved.

A much better image of the spectrum is shown on No. 4 plate taken 19 seconds before the contact; here the width of the cusp at its centre was 7·6″, or 0·013 inch on the plate. The focus in all the images appears to be as good as could be wished.

The ultra-violet prismatic camera gave two images of the cusp spectrum, at 34 and 30 seconds before second contact. The spectra are beautifully defined and in perfect focus over the whole length of spectrum photographed, from H_β to λ 3400. All the plates in both instruments exposed later than 19 seconds before second contact are blanks.

In the ultra-violet cusp spectra the thickness of the cusp at its centre was 13·4″ and 11·9″ respectively, but, owing to the comparatively short focal length of the camera, the Fraunhofer lines are narrow and very sharply defined; they have nearly, but not quite, the same intensity as in the normal solar spectrum and differ in this respect from similar spectra taken with the same instrument in 1898, in which the lines, although beautifully sharp, are much weaker than in the ordinary solar spectrum; in these, however, the cusp was but 5″ in width.

It appears that within a few seconds of arc of the limb the intensity of the dark lines falls off rapidly, and shaded lines, such as H and K, lose this character more or less completely and become narrow lines. But there is evidence of a certain amount of variation at different parts of the limb, probably caused by the presence or absence of extended areas of faculæ.

In plates taken in 1900 in the south polar region, where no faculæ could have been present, the dark lines are quite strongly impressed, even on the narrow bands of photosphere spectrum, where the thickness could not have exceeded 1″.

The general falling off in the intensity of the lines close to the limb seems to be simply explained on the supposition that the entire photosphere consists of innumerable vertical columns of incandescent gases partly penetrating the absorbing layer, the lower portion of which would be entirely hidden at the limb owing to foreshortening.

Total Solar Eclipse, 1905, *August* 29–30. *Preliminary Report of the Observations made at Guelma, Algeria.*

By H. F. NEWALL, F.R.S.

(Received November 17, 1905.)

CONTENTS.

§ 1.—*Introductory. Choice of Station.*

The expedition to which this report refers was one of those organised by the Joint Permanent Eclipse Committee of the Royal Society and the Royal Astronomical Society; it was supported by a grant made by the Government Grant Committee.

Guelma was chosen for the site of the observations, as being an inland station between Sfax, which was selected for an expedition from the Royal Observatory, Greenwich, and Philippeville, which it was at first expected Sir Norman Lockyer would occupy. Guelma is 58 kilometres from Bona, 65 kilometres from Philippeville, 55 kilometres from the nearest coast of the Mediterranean Sea; it lies at a height of about 1200 feet above sea-level on the south side of the Valley of the Seybouze, amongst hills which range in height from about 3100 feet at 13 kilometres to the north, to about 4700 feet at 11 kilometres on the south, where lies the celebrated mountain, Mahouna,

"the sleeping lady," so called from the resemblance of its silhouette to the form of a woman. (For the position of the observing hut, see p. 59.)

At the end of November, 1904, I had made enquiries from M. Trépied, the Director of the Algiers Observatory, about the meteorological conditions to be expected in Eastern Algeria at the end of August. M. Trépied replied in a way that recalled very vividly the cordial welcome which he extended to Professor Turner, Mr. Wesley, and myself at Algiers on the occasion of the total eclipse of the sun, 1900, May 28. The information, which he kindly collected from the Service Météorologique de l'Algérie, encouraged the idea that Guelma was a desirable station to occupy, and, though the chances of perfectly favourable conditions at the end of August were not high, yet, when expressed in statistical form, they appeared vastly better than our actual experiences in the week which included the eclipse day. The following record of weather conditions about 1 to 2 o'clock (the hour of the total phase of the eclipse was two o'clock), makes it clear that the expedition was very fortunate:—

Friday,	August	25.	Storm, rain and cloud.
Saturday,	"	26.	Sky completely clouded.
Sunday,	"	27.	Violent thunderstorm and dust storm.
Monday,	"	28.	Sky completely clouded.
Tuesday,	"	29.	Sky considerably veiled by cloud.
Wednesday,	"	30.	Superbly clear sky (eclipse day).
Thursday,	"	31.	Sky completely clouded.

Fortunately at other hours on most of these days there was plenty of sunshine available for the adjustment of the various instruments. The record for the previous fortnight that we were at Guelma was much more favourable, with something like 11 clear days and three clouded about the eclipse hour.

I accepted M. Trépied's invitation to share the opportunities afforded by the municipal authorities of Guelma, and to install our instruments in the grounds of the Boys' School of the Commune; and the observations were accordingly made within a few yards of the installations of the Algiers expedition under M. Trépied assisted by Messieurs Rambaud, Sy, and Renaux, and of the Marseilles expedition, consisting of Messieurs Stephan and Borelly, assisted by Dr. P. Stephan.

The school enclosure, with its shady trees and its capacious class-rooms and covered promenoirs, which M. Bachotet, the genial director, encouraged the various expeditions to use, proved an admirable site not only for the eclipse operations, but also for the undisturbed preparation and adjustment of the instruments.

§ 2.—*Facilities.*

The expedition is indebted to the French authorities for facilities in taking the instruments into Algeria free of duty and of inspection at the Custom House; to the Sous-Préfét de l'Arrondissement, and to the municipal authorities of Guelma for their kind permission (strengthened by every friendly attention to further the objects of the expedition) to install the instruments in the yard of the École Communale; to M. Joly, Délégué financier; to M. Trépied, the Director of the Algiers Observatory, for his ever ready aid; to M. Bachotet, the Director of the School, who devoted his summer vacation to assisting the expeditions to utilise the school premises to the best advantage; to the Peninsular and Oriental Steam Navigation Company for their liberal arrangement by which the cases of eclipse instruments were transported free of charge from London to Marseilles and back.

§ 3.—*Personnel.*

I have great pleasure in acknowledging an unusual amount of volunteer assistance in the eclipse operations. To Mr. Lewis Wallace I am under great obligations, not only for the admirable way in which he carried out his important part in the programme of observations on the day of the eclipse, but also for his unstinted assistance in setting up the instruments. He travelled out to Algeria as a volunteer, and his aid from the beginning (August 8) of the preparations until the completion (September 12) of the repacking of the instruments was invaluable, all the more so on account of his experience in Sumatra in the eclipse of 1901.

Mr. H. H. Champion, of Uppingham, joined our party at Guelma on August 24, and gave valuable help in the final preparations. Rev. A. H. Cooke, Headmaster of Aldenham School, and Mr. J. Mello Wadmore, M.A. (Oxon), F.C.S., Science Master at Aldenham School, arrived at Guelma on August 28, and at once took part in the rehearsals of the eclipse operations, in which Mrs. Newall also joined. It is a great pleasure to acknowledge my indebtedness to all for their valuable assistance. The part that each took in the observations will appear later on in this report.

§ 4.—*Itinerary and Summary of Diary.*

July	28.	Departure from England on board of the P. and O. ss. "Egypt," *viâ* Gibraltar to Marseilles.
August	3.	Arrival at Marseilles.
"	4.	Arrangements about transport of instruments.
"	5.	Left Marseilles with instruments on ss. "Ville d'Oran."

August 6. Arrival at Philippeville, 6 P.M.
„ 8. Arrival at Bona, 4 A.M., and at Guelma, 6 P.M.
„ 9. Instruments delivered at the École Communale. Site chosen.

The following three weeks were very fully occupied with setting up the pillars, the tent, and the instruments. In spite of the complete preparation of the instruments before they left England, the time allowed for setting them up in adjustment was found full short; two more days after the rehearsals would have been a welcome addition to our time of preparation, in allowing leisurely final arrangements of the various items of the programme. The day temperatures were commonly over 90° F., and several times over 105°, twice 109° was recorded. The air was usually very dry.

August 28. First rehearsals in afternoon.
„ 29. Rehearsals in morning, afternoon, and in twilight and in the dark.
„ 30. The day of the eclipse. Weather conditions perfect.

Shortly after the eclipse, I despatched the following telegram to the Royal Society :—

"Superb weather conditions at Guelma. Observations successfully made. Brilliant corona. Polar streamers remarkably long, extending towards Mercury over three degrees. Corona of maximum sun-spot type, but unusual dark rays. Splendid prominences."

The next week was devoted to developing all the photographs taken during the eclipse, and in taking subsidiary photographs and observations requisite for the proper interpretation of the eclipse results.

September 5. Dismantling and repacking of instruments begun.
„ 11. Cases closed and despatched *viâ* Bona for Marseilles.
„ 12. Departure from Guelma *viâ* Constantine for Philippeville.
„ 16. Arrival at Marseilles. Arrangements about transport of instruments to London.
„ 18. Arrival at Cambridge, with eclipse photographs.
„ 27. Delivery of instruments at Cambridge, in sound condition.

§ 5.—*The Position of the Observing Station.*

In the list of observatories given in the 'Connaissance des Temps' the position of the mosque at Guelma, as determined in the trigonometrical survey, is :— Latitude, 36° 27′ 55″ N.; longitude, 5° 5′ 34″ (0 h. 20 m. 22·3 s.) E. of Paris. From the large-scale town map of Guelma, which by the kindness of the municipal authorities I was able to study, the site of the observing hut was

found to be 400 metres from the mosque in the N.E. quadrant, and the northerly and easterly components were respectively 76 metres and 393 metres; whence the angular values are found to be 2″·5 and 15″·9. Thus, taking the difference of longitude between Paris and Greenwich as 2° 20′ 14″·4 (0 h. 9 m. 20·9 s.), we have for the position of the hut where the observations were made :—

Latitude ...... 36° 27′ 57″·5 N.
Longitude ... 7° 26′ 4″·3 } E. of Greenwich.
0 h. 29 m. 44·2 s. }

According to the details given in the Eclipse Circular (No. 19) of the Nautical Almanac Office, the line of central totality would pass about 2 miles to the south of the observing station.

§ 6.—*Time Observations.*

By the kindness of M. Trépied and M. Rambaud I was able to compare my chronometer watch (Frodsham, 1862) with the chronometer of the Algiers Expedition. M. Stephan also was good enough to compare my watch with his chronometer (Vissière, No. 63) just after first contact, which I did not observe. These comparisons gave the following results for the error of my watch :—

			Error.	
		h.	m.	s.
(i)	August 30.	0·8	−3	47·4
(ii)	„	5·2	−3	49·4
(iii)	„	23·2	−3	50·9

The following table gives a comparison between predicted and observed times of the contacts, the "predicted" being calculated from the 'Nautical Almanac' :—

	Position angle.	Guelma mean time.	
		Predicted.	Observed.
	°	h. m. s.	h. m. s.
First contact......	296	0 48 56	—
Second „	114	2 8 49·6	2 8 22
Third „	297	2 7 23·8	2 7 4
Fourth „	115	3 20 22	3 20 7

The observed time of second contact is that when the signal, "Go," was given by Mr. Wallace, who was observing the disappearing crescent on the

blue-glass focussing screen of the Dallmeyer telephotographic camera. My exposures with the grating camera at recorded moments give results which lead me to believe that the signal was correctly given.

The observed time of third contact is that recorded by Mr. Wallace when he was looking at the corona through a direct-vision prism, and saw the bright crescents disappear. It seems to me that this time may possibly be five seconds late.

The fourth contact was observed by me through the theodolite attached to the cœlostat, Mr. Champion recording with my watch. I think my call was between 5 and 10 seconds late.

§ 7.—*General Description of the Installation.*

Provision was made for making (A) a few visual observations, but the chief work was (B) the collection of photographic records of various phenomena during the total phase of the eclipse:—

A. The visual observations were to relate—(i) to the extensions of the longer streamers, and special attention was to be devoted to extensions in the direction of the planet Mercury, which the ephemerides showed would be at a distance of about 4° from the sun in a direction not far removed from that of the prolonged projection of the solar axis (for details, see p. 62); (ii) to the plane of polarisation of the earth's atmosphere during the eclipse as determined by means of a properly mounted Savart polariscope; these observations were to be utilised for setting the polarisers used for the photographic records (for details, see p. 63).

B. The photographic work all involved the use of clockwork for giving stationary images of the corona. And, as two clocks were utilised, the installation may be best described in general terms as consisting of two divisions—(i) a powerful spectrograph (see p. 64) and (ii) a set of seven varied instruments, all fixed closely together, and pointed in the proper direction towards a cœlostat, which, being driven by clockwork, gave a stationary image of the corona in each of the instruments. The first instrument was devoted to an attempt to photograph the spectra of the eastern and western equatorial regions of the corona in order to provide means for deducing the velocity of rotation of the corona. The object of the second group of instruments was to get—(i) photographic records of the forms and extension of the streamers of the corona, and also to provide material for a study of the brightness of the corona at different distances from the sun, in continuation of work which was inaugurated by the Committee several years ago, and to which Abney, Thorpe and Turner have mainly contributed by their researches; (ii) photographic records of the phenomena of polarisation

of the corona by three modes of attack; and (iii) photographic record of the flash spectrum under very high dispersion by means of an objective grating spectrograph.

Preliminary notes of each of these lines of investigation will now be given.

§ 8.—*Visual Observations. Extension of Coronal Streamers, etc.*

Extension of Coronal Streamers.—The corona was obviously of the type associated with the maximum in the sun-spot cycle. The absence of anything like polar features accompanied by marked equatorial extension of the corona was noted by all the observers. The general brightness of the corona was distributed round the whole limb to approximately the same radial distance except for four or five marked streamers. The longest of the prominent streamers were those which appeared close to the south pole of the sun. The estimates of extension differed very much :—

Observer.	Estimate of extension of southern streamers.	Stars and planets seen.
H. H. C.	$1\frac{1}{2}$ diameters.	Regulus, not Mercury.
A. H. C.	No estimate.	Mercury seen.
H. F. N.	Four-fifths of way from ☉ to ☿.	Mercury seen, no stars.

Mr. Champion prepared charts showing the relative positions of the sun and Mercury, and some bright stars; and during the eclipse he noted down the general positions of the main streamers. These recorded positions coincide remarkably well with the streamers shown on the photographs. He, like myself, was struck by the fact that the easterly edges of two prominent streamers, one above, the other below the sun, seemed to be in continuous line approximately tangential to the sun's limb—an observation which is not completely borne out by the photographic records. But the extensions indicated in his sketch are in no case greater than $1\frac{1}{2}$ diameters.

For my own part, I devoted my attention to following out the extension of the streamers in the direction of Mercury to their furthest traceable limit. These two streamers, together with one on the east side, at once struck my attention because each seemed to be flanked by a dark edge, which gave the appearance of dark rays in the corona. (The origin of such dark rifts in a three-dimensional corona is a matter for interesting reflection. Filiform shadows or filiform regions free from luminous matter could hardly appear as dark rifts, and we are left with alternatives (1) of rifts of small breadth, but of great depth and length, or (2) of raylike regions of strongly absorptive matter, or (3) of a corona in which the luminosity is, on the whole, confined to rays. Analogous reflections as to the origin of the contrast between the

dark and bright rays often seen at sunset near shade-producing clouds lead one to the simple supposition that the light-scattering particles are mainly confined to a stratum of air at about the same height as the clouds.)

When thus carefully followed the streamers could be traced very far, till the contrast was far smaller than one would ever hope to catch in a photograph unless it was taken with such precautions as would be needed to portray delicate cirrus cloud. Still I confess it was a surprise to me to find that the furthest extension recorded on any of our photographs is barely $1\frac{1}{2}°$, or, say, 3 diameters, as compared with my visual estimate, which gives something less than 6 and more than 5 diameters.

§ 9.—*Visual Observations of the Polarisation of the Earth's Atmosphere during Eclipse.*

A Savart polariscope, mounted with pointer and graduated circle, was pointed to the region of the sky near the eclipsed sun, 30 seconds after second contact. I found the polarisation not very strong; in no position of the Savart were the bands very marked. I was prepared to find vertical polarisation, and the corresponding reading of the pointer when the bands were extinguished by rotating the polariscope in the usual way, would have been 45°, with a white-centred band system at the reading 0°.

The readings were actually 37° in the counter-clockwise rotation.
" " 42° clockwise "

Without assuring myself that the band system was white centred in the vertical position of the instrument, I interpreted my readings to mean that the plane of polarisation was nearly vertical, and adjusted the photographic instruments (p. 70) accordingly.

Now the actual mean reading $39\frac{1}{2}°$ meant that the plane of polarisation was inclined either at $5\frac{1}{2}°$ to the vertical, sloping downwards to the left hand, or else at $5\frac{1}{2}°$ to the horizontal, sloping downwards to the right. Unmistakable evidence afforded by the photographs shows that the atmospheric polarisation was in the nearly horizontal plane.

The photographic evidence, moreover, shows that at Guelma the atmosphere diffused as much polarised light as was emitted by the corona at a distance of about $1\frac{1}{2}$ diameters from the moon's limb. (See p. 71.)

These two facts, so important in the study of the polarisation phenomena of the corona, make me regret that by an oversight, resulting from an accident before leaving England, I had omitted to pack the set of five Savart polariscopes prepared for a complete survey of the atmospheric polarisation on lines similar to those in part carried out by Mrs. Newall at

Algiers in 1900. The oversight was discovered a week before the eclipse, and though the instruments were sent out in response to a telegram and arrived two days before the eclipse, it was judged impossible in the then condition of the sky to provide for the rehearsals needed for a proper set of observations.

With reference to the intensity of atmospheric polarisation observed by me at four recent eclipses, the following summary is not without interest:—

1898.	India.	Polarisation	intense (dry air).
1900.	Algiers.	„	fairly strong (on the sea-board).
1901.	Sumatra.	„	imperceptible (moist air).
1905.	Guelma.	„	rather weak (dry air).

If one may hazard numbers on memories I would mark the intensities 10, 7, 0, 3.

There is much to be said for the view that the atmospheric polarisation during eclipses is in great measure due to the fact that the atmospheres diffuses the polarised light of the corona, which in general must have a determinate resultant plane of polarisation, depending upon the distribution of brightness in the radially polarised corona.

Photographic Work.

§ 10.—*Four-prism Spectrograph with Two Slits.*

The same four-prism spectrograph, with two slits, as was used in 1901 in Sumatra and as was described in the 'Proceedings,' vol. 69, p. 220, was used in an attempt to photograph the spectrum of the eastern and western equatorial regions of the corona simultaneously, for a determination by a spectroscopic method of the velocity of rotation of the corona. The only change in the instrument consisted in the piercing of windows in the sides of the tube just in front of the slits, in order to admit of illumination of the slits, if required.

No results were obtained, for the light of the corona at 3′ (minutes of arc) from the sun's limb failed to impress the photographic plate. [Mr. Dyson informs me that in his photographs of the coronal spectrum, obtained at Sfax, photographic action on the plate is not seen at greater distances than $3\frac{1}{4}'$ from the sun's limb.]

Nothing in the preparations or during the eclipse itself was omitted which could have made the attempt successful. The subsidiary photographs of the spectrum of the light of the sky, obtained immediately before and after the eclipse, showed that the instrument was in perfect order. The

image of the corona on the slits looked splendidly bright; the driving of the clockwork was excellent, thanks to the great care that Mr. Champion was good enough to devote to the rating of it. I felt convinced that success was achieved, but in spite of all efforts in development of the plate, no trace of spectrum was obtained.

I decided not to dismount the instrument immediately after the eclipse, but to repeat, in the conditions at Guelma, some experiments made several years ago to test the limits of the light-grasping power of the instrument. Accordingly, a week later, photographs of the spectrum of the moon at first quarter were obtained with various exposures from 20 seconds to 180 seconds; they gave results which, taken in connection with the results of the photographic investigations of the polarisation of the corona, prove that the use of so powerful a spectrograph was based on too sanguine hopes as to the brightness of the corona at 3′ from the sun's limb, even under the exceptionally favourable conditions experienced at Guelma. I am led to infer, from the strength of the spectrum obtained on September 5 by an exposure of 20 seconds to the moon's limb at first quarter, that the unpolarised light of the corona at 3′ from the sun's limb cannot be as bright as one-eighth of the brightness of the moon's surface. Turner's law of brightness, which refers to the total (polarised and unpolarised) light of the corona, would show that the brightness at 3′ is one-third of the brightness at the sun's limb.

§ 11.—*The Cœlostat and the Fixed Cameras.*

The cœlostat has a mirror of silvered glass 16 inches (40 cm.) in diameter, worked by the late Dr. Common, to whom eclipse observers are so much indebted for his development of the cœlostat method. The mounting was made by Cooke and Sons, of York, and the instrument was used to supply light to seven fixed cameras described in the following paragraphs. This instrument has been constructed with a view to its being mounted for solar observations at Cambridge. By a fortunate coincidence it happens that the latitude of Cambridge is little different from the co-latitude of Guelma, and thus it was possible to design the instrument so that it would serve for observations in Algeria if the wedge-shaped casting that supports the polar axis on its hypotenusal face were overturned, the side that is to be vertical at Cambridge being laid approximately horizontal at Guelma. Mr. Wallace succeeded admirably in rating the excellent clockwork supplied with the instrument.

The incidence of the coronal light on the mirror was arranged to be approximately 15°. Thus the reflected beam was of elliptical section with

minor and major axes respectively 15 and 16 inches. The small incidence of the light on the silvered glass could not give rise, as was proved by direct experiments of a delicate nature, to polarisation effects of a kind likely to interfere with the polariscopic observations.

The azimuth and altitude of the sun were calculated to be 53° 52′ and 50° 38′ respectively, at the time of mid-totality. The various instruments which were to be supplied with light were fixed parallel to one another on a brick pillar, and were pointed downwards towards the cœlostat in a direction whose azimuth and altitude were 60° and 21° 10′ respectively on the day of the eclipse.

The sun's declination was changing from day to day by amounts that made it impossible to fix the instruments at the outset in their final position, without forfeiting the chances of utilising the sunlight for adjusting purposes. Accordingly the faggot of instruments was built together on the top of the rectangular box known as the double tube, which served as a camera with partitions in it for three of the instruments. The whole faggot was then bodily moved on the fixed pillar from day to day to suit the sun's declination, and it was firmly fixed in final position on the morning of the eclipse.

The following instruments were supplied with light from the cœlostat:—

1. The Dallmeyer objective of 4 inches aperture used with a negative lens to give a magnified image of the corona for measurement of the brightness: diameter of the moon 1·5 inches, on plates 6½ inches × 6½ inches. (See § 13.)

2. A Cooke objective of 1·5 inches (37 mm.) effective aperture, giving images of the corona for comparison with the polarised images taken simultaneously with the Savart camera (No. 3), and with the Nicol camera No. 4; diameter of the moon 0·4 inch (10 mm.). (See § 14.)

3. The Savart camera of 1·5 inches (37 mm.) effective aperture, with a large Nicol prism and Savart plates, each 14 mm. thick, in front of the objective, to give an image of the corona with Savart bands for investigation of the polarisation of the corona. (See § 14.)

These three instruments were arranged in the "double tube" which was provided with nine double plate-holders, each carrying two plates (6½ inches × 6½ inches) which could be exposed simultaneously by a quarter turn of one shutter; one of the two plates in each exposure received two images, separated along a diagonal and impressed by cameras Nos. 2 and 3 respectively.

4. The Nicol camera, consisting of a Cooke photographic objective of 2 inches (51 mm.) effective aperture and 66·8 inches (170 cm.) focal length,

used with an exceptionally large and fine Nicol polarising prism which transmitted a 2-inch beam and gave an image of the moon 0·65 inch (17 mm.) in diameter. (See § 15.)

5. A photographic objective-grating camera with a Rowland grating of ruled surface 5 inches × 3½ inches (126 mm. × 90 mm.) and 14,438 lines to the inch, in front of a 4-inch photovisual object-glass of focal length 72 inches, by Cooke and Sons of York. This camera was supplied by light from the cœlostat beam by means of an auxiliary silvered mirror which reflected the light in such a way as to give the crescents conveniently disposed with respect to the plane of dispersion. The axis of the camera was below the main faggot of instruments and at right angles to its length. (See § 16).

6. A polarising spectrograph, arranged in such a way as to throw two polarised images of the spectrum of the corona side by side on the same photographic plate, for comparison of the tangential and radial components of the coronal light at different parts of the spectrum. (See § 17.)

7. A simple camera of short focus (11 inches) and aperture $f/7·5$ to give a small picture of the corona with long extension of streamers. This was supplied by a totally reflecting prism, which was set in the cœlostat beam and sent the light downwards in a vertical plane. (See § 18.)

The accommodation of these instruments within a single hut, covering a ground plan 10 feet 6 inches × 15 feet 2 inches, needed careful arrangement. Four pillars were built, one for the cœlostat, another for the four-prism spectrograph, a third for the fixed cameras, and the fourth for the objective grating camera.

§ 12.—*The Signals for the Operations During the Eclipse.*

The signals for the operations were given partly from chronometer readings, but chiefly from observed length of the disappearing crescent of the photosphere. Seconds were counted aloud in time with the beats of a carefully adjusted metronome. Each observer recorded the epochs of his operations in terms of the count, except myself, for whom Mrs. Newall recorded.

All observers were at their stations at attention for several minutes before second contact. Mr. Wallace, who had charge of the double tube camera and was watching the image of the disappearing crescent upon the blue focussing plate of the Dallmeyer telephotographic camera, called out the signal "Stand by," when the length of the crescent fitted a template which I had supplied him with and which was calculated to give the length of the crescent at 32 seconds before second contact. At the signal, Mr. Champion began

counting with the metronome and continued it until the signal "Go" was given by Mr. Wallace at the moment of disappearance of all photospheric light. This occurred just after Mr. Champion called "29." Then Mr. Wadmore began a fresh count from 1 up to 60 and again from 1 to 60. Next Mr. Champion renewed the counting through the third minute in intentionally monotonous voice, the entry upon the fourth minute being indicated by a change of voice, which was to denote that the last 35 seconds of totality were passing. He continued counting after third contact, until released by myself, who, after the operations for the second flash spectrum, compared his count with the chronometer, so as to get the chronometer reading corresponding with the count at any moment during totality.

Each observer was instructed as to the duration of the desired exposure in his programme. He noted the count at the beginning of each exposure on a paper on which the duration was already recorded, and, by adding the two numbers, he knew up to what count he had leisure to look at the corona. Thus an exposure of 30 seconds began at count 37: the numbers were recorded, and leisure terminated at 7.

Mr. Wallace had charge of the double tube cameras. Mr. Champion set the Savart to the reading, which I gave to him after making visual observations of the atmospheric polarisation; and he also made the exposures for the polarising spectrograph. Mr. Cooke carried out the programme for the Nicol camera, after I had set the graduated scale in the proper position in accordance with the visual observations; his operations consisted in setting the Nicol prism and also making an exposure of 30 seconds, four times during the eclipse. Mr. Wadmore made a long exposure with the short focus camera. Mrs. Newall recorded the epochs of opening and closing whilst I was manipulating the grating camera; and she secured two photographs during totality with the grating camera. My own programme included (*a*) operations with the four-prism spectrograph, viz., taking photographs of the spectrum of the sky about 10 minutes before and after totality and also taking a photograph of the spectrum of the corona during totality; (*b*) operations with the grating camera, involving the focussing of the camera and fine adjustment of the grating to be done about 5 to 2 minutes before totality and making exposures (six for each) for the first and second "flash" spectra; (*c*) making the visual observations of the atmospheric polarisation and then setting the Nicol prism and circle for the exposures with the Nicol camera and also giving the reading for the Savart camera; (*d*) naked eye observations of the extensions of the corona, etc.; (*e*) the exposure to the light of the corona of a photographic plate behind a set of Abney squares for photometric purposes.

All the operations in the programme were rehearsed until each observer

was satisfied that his part was under control. The last of the rehearsals were held on the evening before the eclipse day, both in the twilight and in the dark with lamps and candles, a procedure which was found very helpful as a preparation for the actual eclipse.

§ 13.—*Dallmeyer Telephotographic Camera.*

Nine plates were available for exposure, and were utilised as follows:—

Plate.	Subject.	Exposure. Beginning (Guelma mean time).	Exposure. Duration.	Kind of plate.	Purpose of photograph.
		h. m. s.	sec.		
No. 1	Partial phase...	1 32 25	[illegible]	Ilford Process	For diameter of sun's image.
2	Disappearing crescent	2 0 50	[illegible]	,,	For position angle.
3	Corona	2 3 33	11	,,	For prominences and inner corona.
4	,, ...	2 4 39	16	Ilford Empress	For middle corona.
5	,, ...	2 5 1	32	,,	For outer extensions.
6	(Not exposed)	—	—	Rocket	
7	Corona	2 5 52	16	,, ...	For outer extensions.
8	,, ...	2 6 14	8	,, ...	For inner corona and weak development.
9	Reappearing crescent	2 14 47	[illegible]	Ilford Process	For position angle.

Plate No. 6 was not exposed during totality; it was to be at Mr. Wallace's discretion to expose or not according to the interval needed between Nos. 3 and 4 for the visual observations of the atmospheric polarisation which I was to make. The plate was exposed the day after the eclipse, to get three images of the sun at intervals long enough to ensure the separation of the images; the resulting negative serves to give the direction of the parallel (daily motion) on the plate.

Sir W. Abney's standard squares were impressed on many of the corona plates just before development. The photographic squares used for reduction of intensity are the same as were used by me in Sumatra in 1901, and were made by Sir W. Abney. In all cases the standard candle was set at a distance of five feet from the plate, and was backed by a black cloth screen. The exposures given for the several plates are entered in the appropriate column below, in a table which gives information about development, etc.

Plate.	Subject.	Exposure.	Development.	Standard squares.	Remarks.
		sec.			
No. 1	Partial phase...	½	Metol	None	Reversed sun, ugly plate.
2	Crescent.........	¼	"	"	Ditto, ditto.
3	Corona	11	Metol quinol...	"	Clean negative, extension to.
4	"	16	Metol	80 sec. and 5 sec. ...	Ditto, ditto.
5	"	32	"	80 sec. and 5 sec. ...	Clean negative.
6	(Not exposed)				
7	Corona	16	"	120 sec. and 10 sec.	Long streamer. Sky begins to show.
8	"	8	Weak metol ...	60 sec. and 5 sec. ...	Clean negative, detail in middle corona.
9	Crescent.........	¼	Metol	None	Reversed sun, ugly plate.

No. 3 shows that the prominences (so brilliant in the naked-eye view of the corona) form a long bank, reaching over 30° round the sun's limb, northwards from the equator, and extending about 2′ from the limb radially. The lower corona in its neighbourhood is full of complicated structure.

The other plates show a corona of the type connected with the epoch of maximum in the sun-spot cycle; markedly long streamers are shown, stretching out near the south pole; each of these appears as a double bright ray, with a dark ray in the middle. But the most marked appearance of a dark ray is that on the southerly edge of the longest streamer on the eastern limb of the sun.

§ 14.—*Savart and Comparison Cameras.*

These two cameras were included in one compartment of the double tube, and, as the exposures were made simultaneously with those of the plates in telephotographic camera, there is no need to repeat here the epochs of the beginning of each exposure.

Notes as to the kind of plate, and the exposures for the standard squares, are given on the next page. Metol was used as developer in every case.

The Savart and Nicol prism were reset between Nos. 3 and 4.

Plate No. 6 was exposed on the day after the eclipse, for three images of the sun in each camera, for the purpose of getting the direction of the parallel on the plate.

Nos. 8, 4, and 5 were taken with durations of exposure in geometrical progression, so that the polarised image on No. 4 might be compared with the unpolarised image on No. 8, and so on.

Plate.	Subject.	Exposure.	Kind of plate.	Standard squares.	Remarks.
		sec.			
No. 1	Partial phase...	$\frac{1}{5}$	Ilford Process...	None	Reversed sun.
2	Disappearing crescent	$\frac{1}{5}$	,, ...	,, ...	Reversed crescent.
3	Corona	11	Ilford Empress	20 sec. at 5 ft.	
4	,, ...	16	,, ...	80 sec. and 5 sec.	
5	,, ...	32	,, ...	80 sec. and 5 sec.	
6	(Three sun images for each)	$\frac{1}{2}$	Rocket	None	Reversed sun.
7	Corona	16	,, ...	120 sec. and 10 sec.	
8	,, ...	8	Ilford Empress	80 sec. and 5 sec.	
9	Reappearing crescent	$\frac{1}{2}$	Ilford Process...	None	Reversed crescent.

No. 7 was the most sensitive plate used, and the development was somewhat forced, in order to bring out faint extensions of the corona. It has turned out to be one of the most interesting plates of the series, for the bands due to the atmospheric polarisation are visible both above and below the sun. These bands are out of step in the band system over the corona, and prove conclusively (account being taken of the visual observations, p. 63) that if the polarisation of the corona is radial, the polarisation of the sky just above and below the sun was nearly horizontal at Guelma. Moreover, it is seen on the negative that the two band systems neutralise one another, and cease to be perceptible along two linear regions above and below the sun at a distance of about $1\frac{1}{2}$ diameters from the sun's limb. When taken in connection with the visual observations (p. 63) the interpretation of this appears to be that at those points the (approximately) horizontal polarisation of the atmosphere is equal in intensity to the radial polarisation of the corona.

On no other plate of the whole series do there appear any bands due to *atmospheric* polarisation, though by inadvertence, due to a change of plan in my procedure in the matter of adjusting both the Nicol prism and the Savart system, I had given Mr. Champion an instruction to set the Savart system to a reading which differed by 45° from that which I had really intended. By this inadvertence the Savart camera was not used in the way which I consider best: for it was set so as to give the atmospheric bands their maximum intensity instead of extinguishing them. Fortunately, the inadvertence was a blessing in disguise; for it has proved to be the means of making certain about the plane of polarisation of the sky, and also of finding a sort of quantitative relation between the relative amounts of polarisation in the sky and the corona.

With regard to the photographs actually obtained I would point out that,

in spite of the fact that the band system due to the sky must act in a way to diminish the visibility of the band system seen over the corona, yet all the plates show the bands very markedly over the corona, and will consequently form excellent material for a quantitative study of the relation between the polarised and unpolarised portions of the light of the corona; and the interest in this study is heightened by the fact that observations were secured with the same instruments at the epoch of minimum in the sun-spot cycle.

§ 15.—*The Nicol Camera.*

A Nicol prism, with a clear circular aperture of 2 inches, was mounted in front of a photographic telescope of 4 inches aperture and 66·8 inches focal length, the effective aperture being 2 clear inches. The prism was mounted in the usual turning-stand, and a graduated card was fixed so that the Nicol prism might be turned through 135° in three steps of 45° each. The programme was to find the direction of polarisation of the sky, and then to set the Nicol prism in four different positions, namely, parallel and perpendicular to the sky polarisation, and in two other positions each inclined at 45° to the sky polarisation; and in each of the four positions an exposure of 30 seconds was to be given. For convenience in description I shall call the four positions of the Nicol prism 45° E., Vertical, 45° W., and Horizontal, these names indicating the approximate positions of the longer diagonal of the section of the Nicol; the exact positions have been recorded, but need not be here reproduced.

Mr. Cooke made the following exposures:—

Plate.	Position of Nicol.	Duration.
No. X 1.	45° E.	30 sec.
X 2.	Vertical.	„
X 3.	45° W.	„
X 4.	Horizontal.	„

The plates were all of Seed's make, "Gilt Edge No. 27." They were developed simultaneously with metol, and in all processes of treatment have been treated simultaneously. No. X 4 was, unfortunately, not completely exposed before sunlight had reappeared, and, though it turns out to be an interesting and fantastic picture, it is useless for comparison with the others.

Nos. X 1, X 2, and X 3 are all excellent plates, and show very considerable detail in the corona, quite apart from the polarisation phenomena exhibited in them.

If the pictorial aspect of the plates is rather spoilt by the appearance of certain rays and some false images, due to internal reflections in the Nicol prism, it is to be hoped that their presence will not much interfere with measurement. In one respect the rays may be conveniently utilised, namely, for exact determination of the position of the principal plane of the Nicol with respect to the sun's axis.

The negatives render obvious to the most casual inspection the radial nature of the polarisation of the corona.

They also give a conclusive piece of evidence as to the plane of polarisation of the sky. For the shadow cast by a diaphragm in the camera on to the photographic plate leaves the margin of the negatives absolutely clear; whilst the actinic power of the sky has impressed itself faintly in all three plates. It is evident that the effect of the sky is smaller in X 2, when the Nicol was approximately vertical, than in X 1 or X 3, when the Nicol was respectively 45° E. and 45° W. This, taken in connection with the visual observations (p. 63), proves that the sky polarisation was nearly horizontal.

It is, perhaps, rash to speak before having carried out actual quantitative measurement. But a comparison of X 1 and X 3 shows at once a very marked feature. The long streamer which seems to cut across the sun's limb just to the east of the south pole of the sun is very strong in one and almost obliterated in the other; whereas two curved arches, not very far away in the middle corona, remain about equally strong in both pictures. It is very tempting to think that there may be a selective action of the kind. For the researches of Christie, Dyson, and others seem to connect some of these curved arches with outbursts of prominences, and it is not altogether improbable that a change in inherent luminosity of the tenuous matter in the neighbourhood of a prominence might be propagated through the matter in such a way that, when the surfaces were seen in projection, the envelopes were in a sense marked out as apparent arches round the prominences.

What the cause of polarisation in the straighter streamers may be, and, indeed, whether we are to connect marked polarisation chiefly with straight streamers, are questions that would be much elucidated, could we get unimpeachable spectroscopic evidence as to the nature of the light. An attempt of the sort is referred to in § 16, but it is incomplete. If Fraunhofer lines are found in the streamers more strongly than elsewhere, this might be taken as evidence that the polarisation is due to reflection from small particles.

§ 16.—*The Photographic Objective-Grating Camera.*

The Rowland grating used in these observations is part of the spectroscopic installation arranged by the late Professor Piazzi Smyth. I am under great obligations to the Royal Society for the continued loan of this outfit. The grating is a plane grating with a ruled surface 5 inches × 3½ inches on speculum metal, with 14,438 lines to the inch, in all about 72,000 lines. It was fitted on a horizontal axis in front of a telescope of 4 inches aperture and 72 inches focal length, the lens being a very fine photovisual triple object-glass by Messrs. Cooke and Sons, of York.

The programme was to attempt to get: (i) photographs of the flash spectrum with a high dispersion from the green end of the spectrum including the green coronium line to the ultra-violet near K, the photographs to be attempted both at second and at third contacts; (ii) photographs of coronal rings in the same region of the spectrum of the corona during totality.

The breaking of a string used in these operations about 4 minutes before second contact rather upset the even tenor of things, but as photographs were successfully obtained, though not exactly in the form I had originally intended, I need not dwell upon this mishap further than to say that under the stress of the mishap I unfortunately gave way to a doubt as to the correctness of the preliminary rough setting of the grating, and in resetting it I made the very mistake which I thought at the moment I was correcting; for I threw into the camera the *magnified* spectrum of the second order—that is the spectrum got with small incidence and large emergence—instead of the brighter and less dispersed spectrum of the second order, which I had intended to photograph. However, in spite of this mistake, and in spite of the fact that the grating was not used to the best advantage even for that spectrum, the spectrum is satisfactorily recorded on the photographic plate, with a linear dispersion of about 0·4 mm. per tenth metre, over the region of spectrum comprised between the wave-lengths λ 4610 and λ 5120, which thus extends over about 21 cm. or 8½ inches, probably a greater dispersion than has ever been attempted for the flash spectrum.

Three sets of photographs were obtained, each on a 10-inch × 8-inch plate, which was mounted in such a manner that it could be pushed along in its own plane by an inch at a time, and thus as many as seven spectra, each about 9 inches long, could be set side by side on the same plate. A hinged shutter in the camera was closed while the movement of the plate was being made.

The Plate G 1 has on it six spectra of the disappearing crescent. They

relate to the following epochs recorded by Mrs. Newall in terms of the metronome count, whilst I made the exposures :—

No. of spectrum ...	1.	2.	3.	4	5.	6.
Count	17—18	20—21	23—24	27—28	29—2	7—9
Duration of exposure	1 sec.	1 sec.	1 sec.	1 sec.	2 sec.	2 sec.

Mr. Wallace gave the signal "Go"—to announce the beginning of totality —just after Mr. Champion had counted "29." Hence the spectrum No. 5 on this plate was exposed for about half a second to some remnant of photospheric light and for 1½ second to the flash; and the photographic record is quite in accordance with the view that the signal was given at exactly the right moment. Spectra Nos. 1 to 4 show the dark photospheric lines in crescent shape and also a very broad and bright H_β crescent. No. 5 is full of bright and well-defined crescents. Spectrum No. 6 shows only the H_β line.

The Plate G 2 has on it two spectra of the corona, one with exposure of 6 seconds, from the count 15 to 21, the other with exposure of 159 seconds from the count 21 to the end of the third minute, both exposures being made by Mrs. Newall. No coronal rings are visible in the range of spectrum photographed.

The Plate G 3 was exposed by me five times to the reappearing crescent, but, unfortunately, I just missed the flash spectrum by waiting a trifle too long before beginning the exposures.

§ 17.—*The Polarising Spectrograph.*

The spectrograph was a single prism instrument which transmitted a 2-inch beam through the prism. An excellently corrected objective by Cooke, of aperture 2 inches and focal length about 20 inches, was used to throw an image of the corona on the slit. A large double-image prism of square section 2 inches in the side and producing a separation of 3°·3, was inserted between the dispersive prism and the objective of the camera. Had the double-image prism been absent two spectra, due to the two regions of the corona falling on the slit, would have appeared on the plate, with a dark space between them due to the dark body of the moon. The action of the double-image prism was to give two images of these pairs of spectra without any overlapping, and they were arranged so that one was due to the tangential polarised component and the other to the radial.

The programme of exposures was to get two exposures, one of 20 seconds for the inner corona, the other of 120 seconds for the outer corona as well. Unfortunately the second exposure was spoilt by the admission of sunlight.

The short exposure gave a good negative. It was taken on a Seed orthochromatic plate, and shows the spectrum from the yellow to the ultra-violet just beyond K.

The radially polarised images are very much stronger than those tangentially polarised; and this is true for both sides of the corona.

The spectra extend about 5 to 7 minutes of arc from the sun's limb, and fade off gradually to the limit, in such a way that had Fraunhofer lines been present they could readily have been detected. No such Fraunhofer lines can be traced in any of the spectra. In all the four images on the negative, the marked feature is the strength of the continuous spectrum. The lines of coronium are relatively feeble at both points of the corona that fell upon the slit.

There is no trace of light, in either image, across the body of the moon.

§ 18.—*The Short Camera.*

One negative was secured with the short camera of focal length 11 inches, which was provided with a rapid rectilinear lens of aperture $f/7{\cdot}5$. An exposure of about 170 seconds from the count 40 to the count 3 minutes and 30 approximately was given to it, and it was slowly developed until the effect of the sky began to appear on the plate. The longer streamers appear well defined, and show extensions not greater than 90′, or nearly 3 diameters from the limb.

§ 19.—*Development of the Photographs.*

The development of all the photographs was carried out at Guelma in one of the class rooms of the school on the following nights:—August 31, 9 P.M. to 5½ A.M.; September 1, 9 P.M. to 2½ A.M.; September 2, 9 P.M. to 1½ A.M.; and September 3, 9 P.M. to 3½ A.M. The class room was very convenient in both in size and airiness for such operations as imprinting standard squares on the plates. As the temperature was seldom below 80°, plenty of ice was needed, and was fortunately procurable from an ice factory in the town. An alum bath was used for every photograph, and not a single plate was lost.

Preliminary Report of the Expedition to Aswan to Observe the Total Solar Eclipse of August 30, 1905.

By H. H. Turner, D.Sc., F.R.S., Savilian Professor of Astronomy in the University of Oxford.

(Received October 19, 1905.)

1. *General Objects.*—The particular pieces of eclipse work kept steadily in view by the writer during recent years are—

(*a*) The determination of the brightness of the corona by photographic methods at different distances from the sun's limb.

(*b*) The determination of the fraction of this light radially polarised.

Attention was directed to both these objects on the present occasion and in addition an opportunity presented itself of making a trial of—

(*c*) The use of long focus mirrors in eclipse work.

This question of long focus mirrors was brought before the Joint Permanent Eclipse Committee three years ago, and a Sub-committee of three (Dr. A. A. Common, Sir J. N. Lockyer, and Professor H. H. Turner) was appointed to deal with the matter. Dr. Common was making two or three mirrors for trial when his lamented death put an end to the experiments. While the remaining members of the Committee were considering how to proceed, I learnt of the existence of a 2-foot mirror of 120 feet focus, with cœlostat of 28 inches diameter, constructed by Mr. J. H. Reynolds, F.R.A.S., of Birmingham, and mounted in his garden; and I further gathered that he would like to try the instrument in Egypt at the Eclipse of 1905, as he had already another reason for visiting the country about that time. He had, in fact, been so much impressed, on the occasion of two previous visits to Egypt, by the suitability of the climate for astronomical work, and the desirability of having a large telescope there, that he had offered to present to the Helwan Observatory a reflecting telescope of 30 inches aperture and 11 feet 6 inches focal length, the mirror of which was made by the late Dr. Common, F.R.S., and the mounting of which was to be designed and constructed by Mr. Reynolds himself. Before accepting this offer, Captain H. G. Lyons, Director-General of the Egyptian Survey, under which department the Helwan Observatory is placed, did me the honour to ask for my opinion whether such a telescope could be profitably employed in Egypt and what work it could most suitably do. There could be no hesitation in replying to the former question in the affirmative, and as regards the latter, since the low latitude of Egypt brought within reach many new nebulæ not obtainable from observatories already equipped with large instruments, it was obvious that, for some years at any

rate, the particular work of the reflector which the maker of the mirror had himself initiated, could be pursued to the utmost advantage. Captain Lyons accordingly accepted Mr. Reynolds' offer, and at the meeting arranged for the discussion of it, he also gave Mr. Reynolds and myself a most cordial invitation to Egypt for the eclipse, promising that he and the Survey Department would do all in their power to assist us; a promise which he literally fulfilled. Under these circumstances I felt justified in asking the Joint Permanent Committee to sanction the addition to our programme of the trial of this large telescope, which Mr. Reynolds freely put at our disposal; and further to allow Mr. Bellamy to accompany me, since Mr. Reynolds himself could not arrive in Egypt till a few days before the eclipse.

(*d*) Finally I was asked by Mr. E. W. Maunder to take charge of one of a pair of Goerz lenses, which I understand were lent for the expedition by the firm on the suggestion of Mr. A. Reichwein. Mr. Maunder took the other with him to Labrador, and arranged a definite programme for use with both instruments, with a view to stereoscopic combination of the pairs of photographs thus obtained at the ends of the line.

We were fortunate in being able to hand over this instrument, after carefully focussing it in Aswan during the week preceding the eclipse, to the care of Mr. R. T. Günther, of Magdalen College, Oxford, who came to Egypt at his own expense and volunteered to assist the expedition.

(*e*) One or two possible additions to the programme had to be reluctantly abandoned. Especially do I regret that it was not possible to do anything in the direction suggested by Mr. C. E. Stromeyer for accurately observing the positions of the limits of totality on the earth's surface. We found on actually arriving in Egypt that all available energy was used up on the existing programme, and to add anything would seriously endanger the whole.

2. *Personnel.*—The expedition from England thus consisted of the following persons:—

Prof. H. H. Turner.
Mr. F. A. Bellamy. } From the Oxford University Observatory.

Mr. J. H. Reynolds, of Birmingham.

Mr. R. T. Günther, of Magdalen College, Oxford.

But these would have accomplished little without the aid of the following officers of the Survey:—

Captain H. G. Lyons, Director-General.	Mr. E. M. Dowson.
Mr. J. I. Craig.	Mr. J. R. Herbert.
Mr. H. E. Dickinson.	Mr. J. Kearney.
	Mr. B. F. E. Keeling.

Mr. Keeling took charge of all the operations of erecting piers and shelters and similar work which would have been extremely difficult to strangers unacquainted with Arabic. He worked untiringly throughout, and we owe him a great deal, especially for his exertions during the last few days in getting the large telescope mounted at very short notice. Mr. Kearney developed the plates in Cairo, under the admirable conditions available at the Survey Department. All the gentlemen named took an important share in the operations during totality, while Mr. W. M. Aders, who was acting as correspondent for the 'Egyptian Gazette,' also kindly took charge of an exposure, and Mr. Ball helped Mr. Bellamy with his plate-holders.

Of Captain Lyons himself, and what he did, not only for the British Expedition, but for the American and Russian Expeditions, it would be difficult to write adequately. In preliminary suggestions, in arrangements for meeting us on arrival and seeing us comfortably established, in placing the resources of the Survey Department at our disposal, and in actual personal assistance in the work, he was equally generous.

3. *Dates and Itinerary.*—The bulk of the Oxford instruments were placed on the Prince Line Ss. "Trojan Prince" in London, on which Mr. Bellamy took his passage on Monday, July 17. The 13-inch objective of the astrographic telescope was placed in his cabin; and the box containing it was handled throughout the expedition either by him or by Professor Turner. The "Trojan Prince" took longer than usual over the voyage, not arriving at Alexandria till Saturday, August 5.

Mr. Keeling went to Alexandria to meet the boat, and saw the instruments put on the train for Aswan, where we found them on our arrival on Tuesday, August 8.

Professor Turner left Oxford for Marseilles on Thursday, July 27, and there joined the P. & O. steamer "Himalaya," on which had been placed in London two cases of instruments and plates which could not be got ready for the "Trojan Prince." The P. & O. Company not only allowed these to go as passengers' baggage, but courteously carried them free of charge.

Mr. Reynolds was not able to leave England until August, and joined the Orient steamer "Oroya" at Marseilles. This boat Mr. Günther also joined at Naples on August 20, and it arrived at Port Said on Friday, August 25. Mr. Reynolds' large telescope was also on this boat, it having been found impossible to send it earlier owing to circumstances which will be mentioned in §§ 26, etc. Mr. Reynolds arrived at Aswan with his instruments on Saturday afternoon, August 26, just four clear days before totality.

We have to cordially thank the Egyptian railways for transporting us and our instruments from the ports of arrival in Egypt to Aswan, and back again, free of charge.

4. *Station.*—The central line cuts the Egyptian railway close to Khattara in latitude 24° 12′. Captain Lyons was prepared to make arrangements for establishing the expeditions there, either by encampment or by providing dahabeahs; but, on learning that it was not of vital importance to be actually on the central line, he unhesitatingly recommended our taking up a position at Aswan in latitude 24° 6′, where only 3 seconds of totality was lost, and where there were all the resources of civilisation to be had. The large Savoy Hotel was open all the summer, and we were very comfortably quartered indeed.

It was, however, practically impossible to select a site before our arrival. To have done so would undoubtedly have been advantageous in allowing of the erection of piers in time for them to settle firmly; but the available and suitable sites were neither numerous nor large, and the choice was a matter of some difficulty. The managers of the hotel very kindly put their grounds at our disposal, and ultimately sites were chosen in the grounds for all three expeditions. But the hotel is on an island, reached from the railway by boats; and, after some experience of the conveyance of instruments across, and their handling on the island, it was felt that there would be difficulties and delays attending the transport of Mr. Reynolds' telescope thither; and when it became known how short the time available for its erection would be (four days only) it was decided to abandon the site chosen for it in the hotel grounds, and to substitute one at the railway station, where it could be mounted immediately on arrival. On visiting the railway station it was seen that if the cœlostat pier could be erected on the station platform, and that for the concave mirror almost on the public road, the rays between the two mirrors would pass over the intermediate area, which was a sunken garden, at a height of several feet, obtaining the effect of raising the piers to a considerable height. On making inquiry whether permission could be obtained, it was at once given; and thus, by the kindness of the railway authorities, a site having conspicuous advantages was obtained. It was further felt that if the ground could be well soaked the disturbance from radiation would probably be sensibly diminished. While we were debating the best method of doing this, the local fire-brigade happened to be drilling, and Mr. Keeling promptly asked whether they could help us. The affirmative reply was equally prompt, and on the day of the eclipse the ground was flooded three times, at intervals of some hours.

The position of the stations may be taken approximately as—

Latitude .... 24° 5′·5 N.
Longitude ...... 32° 52′·5 E.

An American expedition under Professor Hussey, and a Russian expedition from Pulkowa and Pawlowsk, were established in the same hotel, and erected their instruments also in the grounds; except that Dr. Doubinsky, of Pawlowsk, set up his magnetic self-recording apparatus in a tomb (No. 32 of 'Grenfell's Tombs') on the west bank of the Nile.

5. *Meteorological Conditions, etc.*—The chief characteristic of the weather was, of course, its steadiness. The following figures from the Daily Weather Report, issued by the Survey Department, will sufficiently illustrate this. The observations are made at 8 A.M.:—

Date.	Barometer corrected.	Wind.		Cloud.	Temperature, Centigrade.			Humidity.
		Dir.	Force.		Max.	8 A.M.	Min.	
					°	°	°	
Aug. 5	756·1	N.	Very light	Clear	42	31·6	25	29
6	756·1	N.	Very light	Clear	42	32·4	24	28
7	755·7	N.	Very light	Clear	43	32·2	26	19
8	756·7	N.	Very light	Clear	43	30·4	25	23
9	757·2	N.	Very light	Clear	41	31·2	24	35
10	756·4	N.W.	Very light	Clear	42	32·6	25	26
11								
12	756·3	N.	Very light	Clear	42	32·6	25	27
13	757·2	N.	Light	Clear	41	30·2	26	29
14	756·2	N.	Light	Clear	39	29·8	24	22
15	756·4	N.	Very light	Clear	41	30·6	24	23
16	755·9	N.	Light	Clear	42	32·8	26	21
17	757·1	N.	Light	Clear	42	31·6	26	25
18								
19	755·0	N.E.	Moderate	Clear	42	29·8	26	27
20	755·0	N.	Light	Clear	42	30·4	24	30
21	755·8	N.	Very light	Clear	42	30·4	25	33
22	755·2	N.E.	Very light	Clear	42	30·6	24	29
23	755·7	N.	Fresh	Clear	42	33·6	29	33
24	755·8	N.	Fresh	Clear	42	30·2	27	31
25	756·4	N.E.	Fresh	Clear	40	29·6	26	42
26	756·1	N.	Very light	Clear	40	29·2	24	46
27	756·2	N.	Very light	Clear	40	29·6	23	58
28	756·3	N.	Very light	Clear	40	29·2	25	44
29	756·5	N.	Almost calm	Clear	41	30·2	24	50
30	757·8	N.	Very light	Clear	40	29·2	22	49
31	757·4	N.	Almost calm	Clear	40	29·6	22	51

There was thus a steady north wind, light in the daytime, but often strong at night, and as sand came with it, it was advisable to have screens to the north of the instruments, though nothing really kept the sand out. Fortunately everything was so dry that the sand was soon blown away again, and our clocks suffered far less than might have been expected.

The sky was almost permanently clear, with one notable exception. On the afternoon of August 21 the sky clouded over completely and quite

thickly. The sun was quite invisible from before the time corresponding to first contact until sunset. Had August 21 been the day of the eclipse we should have seen nothing at all. Such facts are worth remembering in connection with eclipses. In 1889 (December) there was considered to be a practical certainty of fine weather in West Africa, and yet it was cloudy for four days at the time of eclipse. It seems doubtful whether anything approaching real certainty of a clear sky can be attained.

We had no rain, and the air was very dry, though the humidity rose sensibly about August 25 to 31. The high temperature was, therefore, not so trying as might be expected. But in many ways we were constantly reminded of it. On dressing we found our clothes hot and dry; metal work was often scorchingly hot, especially if left exposed to the sun for a few minutes: indeed, almost anything might happen in the sun: for instance, a stick of ebonite, which Professor Doubinsky, of the Russian Expedition, stuck in the sand to make an experiment on atmospheric electricity, was soon seen to be bending over, partially melted.

The Nile was steadily rising during our stay at Aswan, though the flood was much below the average.

Insects and small reptiles were very numerous. There were very few mosquitoes or sand flies, and not very many flies, though the few were very persistent. But spiders, lizards, scarabs, scorpions, and other animals swarmed among our instruments, fortunately without doing any particular harm. When the tube of the 13-inch was covered with cloth a scorpion of some size took up his abode therein, and though we often saw him we failed to either catch or dislodge him until the whole was dismounted after the eclipse. Any alterations in the instrument were thus made with some wariness.

There were often clouds on the horizon, but sometimes it was wonderfully clear. We saw a bright star set behind a low hill with all the suddenness of an occultation. On the day following the eclipse (August 31) the horizon was distinctly hazy, but we caught sight of the 26-hour-old moon at 6.45 local time, and photographed it in 3 seconds with the fixed Goerz lens— it being quite out of the reach of our cœlostat and 13-inch. The next night it was easily photographed with both instruments, the earth-shine plainly showing on the pictures. [To give an exposure of 10 seconds the cœlostat was slightly altered for the moon's motion. The altitude of the axis was dropped 47′, and the clock was made to go 2·3 seconds slow in the minute.]

The dry climate was very hard on wood of all kinds. We were warned of this, and in arranging plate-holders I consulted Messrs. Watson and Sons

whether they had not better be in metal; but they expressed confidence that they could make them in wood to stand the climate. The confidence was only partially justified, several of the plate-holders warping considerably; and if this was so in cases where special care was taken it was only natural that worse should happen in other cases. A large amount of time, during the expedition, was consumed in refitting things that no longer fitted at all. Fortunately most of the changes seemed to take place at once and then to be arrested, so that it was possible to make alterations permanent.

The exceptional dryness of the climate and the high temperature naturally brought troubles in developing plates. Solutions evaporated so rapidly that it was difficult to judge of their strength at a given moment. Of course, ice was used plentifully, otherwise development was impossible; but even then it was not easy to work. Sodium sulphite melted, and this did no harm so long as the bottles were well sealed; but when they were merely corked the salt appeared to decompose and gave trouble. All these troubles, however, fell lightly on us who had merely preliminary plates to deal with, and could hand the eclipse plates over to Mr. Kearney's experienced hands. Professor Hussey bore the brunt of them in Aswan.

This last fact had one unfortunate consequence. It was hoped that Professor Hussey might have been able to make a critical examination of the "seeing" at Aswan, which would have been particularly valuable owing to his exceptional knowledge of climates, for it will be remembered that on behalf of the Carnegie Institution, who were contemplating the erection of new observatories, he visited not only Mount Wilson, in California, where the new solar observatory has since been established, but Australia and Tasmania, in search of a site for an observatory in the southern hemisphere, and has thus added largely to his already wide experience at Mount Hamilton. He had not a leisure moment before the eclipse, but afterwards he hoped to make use of the 8-inch objective which Captain Lyons kindly had sent down from Helwan, and we all looked forward to the result of his observations with great interest, as he did himself. But the development of the plates was so anxious and slow a process as to occupy every night available after the eclipse, and he was compelled, with the greatest reluctance, to give up the idea of finding sufficient time to make the tests which he considered necessary to give any information of value.

6. *Piers, Huts, etc.*—Brick piers with a concrete foundation were built under Mr. Keeling's direction. It was impossible to get a rock foundation, and we did the best we could in the sandy soil. In the exceptionally dry climate of Aswan, the difficulty was to keep the mortar and cement wet long enough to give a solid setting, but sufficiently steady results were

obtained. According to a practice found useful in previous eclipses, blocks of wood were introduced into the piers for the attachment of apparatus by screwing down, or for binding with cords round projecting ends. In this case the method was not successful. The wood absorbed any moisture used for the cement and bricks and swelled, cracking several of the piers, but fortunately no serious damage to the observations resulted.

Shelters were necessary because of sun and wind, though not for rain. They were made by covering a skeleton wooden framework with matting arranged in panels. The straw mats were a well-known local commodity, and we found many of the windows of the Savoy Hotel closed up with them during the summer as a protection against the fierce sun. They measured 10 feet × 6 feet. With a simple wooden framework they were rigid enough to be easily placed in position by a single person if necessary, and were heavy enough to remain in place when merely laid on the roof, though the wind was often strong. In the rainless climate they gave all the advantages of a sliding roof at a trifling cost.

The mounting of the large telescope is separately referred to under § 31.

7. *Instruments Mounted in Savoy Hotel Grounds.*—At previous eclipses (1896, 1898, 1900) I had used one of the "double tubes" constructed for the eclipse of 1893, pointed to a 16-inch cœlostat, the polariscopic apparatus being attached to this tube. In 1900, when photographs with the separate components of the "Abney" lenses were discontinued, one half of the tube was set free, and in the other half polariscopic apparatus was arranged by Mr. Newall and myself conjointly. In 1901 Mr. Newall kindly took charge of the same instrument in Sumatra, but finding difficulties with my Iceland spar prism apparatus (as explained in §17), he omitted it and substituted another arrangement. It was an obvious convenience for him to have the same double tube arranged in the same way for the present eclipse, and quite as easy for me to take out another instrument, especially as Mr. Newall kindly handed over to me the 16-inch cœlostat and purchased another for himself. It became, therefore, necessary to decide on (*a*) a telescope for simple photographs of the corona which might also afford material for determining its general brightness in continuation of previous work; and (*b*) apparatus for taking the photographs in polarised light.

The Astrographic Telescope.

8. For the former purpose we decided to bring out the astrographic telescope, or rather the object-glass of it. Our plates for the catalogue being completed, there was no risk of a breach of continuity in the series, and I

was glad to learn that the Astronomer Royal had also decided to take out the Greenwich corresponding telescope.

9. It was decided not to dismount the tube, partly because the dismounting would have been a serious operation and partly because the plate end is arranged for plates 16 cm. square, and it was hoped to use a larger plate for the corona. A new tube was constructed from eight pieces of gas-pipe, each 6 feet in length, with joints so that they could be readily screwed together into four 12-foot rods. These were passed through three rectangular blocks of sequoia wood 2 inches thick, one block at each end and one in the middle. Circular apertures were cut in the middle block and in the end block to which the object-glass was bolted, and a rectangular aperture in the other end block, to admit a box-like structure for the plate-holders. The whole was ultimately wrapped, first with blackened wire at wide intervals, next with black cloth, and finally with canvas. The arrangement worked very satisfactorily, being light and easily transported, and affording means of making the necessary adjustments.

10. *Focussing, etc.*—Each block of wood was held in place by eight nuts screwed on to the gas piping, two nuts on each pipe against opposite faces of the block. For focussing it was only necessary to turn (say) the four inner nuts through a whole turn, or half a turn, and screw the outer ones up so that the wood was again held tight. Screwing the nuts by different amounts on the different rods gave corrections for tilt of plate or objective. Observations for detection of this tilt were made in the case of the plate by putting a silvered mirror in its position, and from the O.G. end measuring the position of an object and its image with a foot-rule in daylight until they were symmetrically placed. In the case of the objective, star images were examined at equal distances on opposite sides of the plate, and the objective tilted until symmetry was obtained. The focussing of the instrument was first adjusted indoors with artificial light by using the cœlostat mirror to reflect the beam back through the objective, and completed by photographs of stars with the tube in position.

11. The box-structure for receiving the plate-holders for 10-inch × 8-inch plates was made of papier mâché, from some spare pieces left from our new dome: and it stood very well, far better than the wooden blocks, which, indeed, did not stand well at all. Sequoia wood was selected as we heard that it made excellent plane-tables for surveying; and it certainly remained flat, but it contracted across the fibre, so that the circular aperture cut for the objective became an ellipse, with minor axis too small to admit the cell. A good deal of scraping away was necessary on more than one occasion to get a satisfactory fit again, and the bolt holes were of course all out of place.

This is one instance of many troubles with woodwork encountered not only by us but by the other expeditions. Small wonder if more than one vow was made never to trust to wood again in future expeditions. And yet I do not feel sure that this is the right inference from our experience. It must be remembered that it was altogether exceptional, and yet that the troubles, though numerous and often apparently serious, were all got over satisfactorily, so far as we know at present. If this can be achieved in an extreme case, it would seem to show that wood can generally be used, and there is no doubt that this means a great saving in weight and expense as compared with metal. But two precautions should certainly be taken in using wood: firstly, to get it as good and well-seasoned as possible, and secondly, to be at the eclipse station as early as possible, so as to give time for correcting unforeseen deviations. As regards the second, we have here only an additional reason for spending more time at the eclipse station, for which course experience has already suggested so many others.

12. The plate-holders themselves were specially made by Messrs. Watson and Sons, of Holborn, with the form of flap-exposing shutter adopted for the "double-tube" holders. Indeed the pattern of these holders, which were made by Messrs. Watson and Sons for the 1893 eclipse, and which have worked admirably at all eclipses since then, was closely followed in the present instance. As already mentioned the woodwork did not offer a perfect resistance to the climate, but it behaved very well on the whole.

The Cœlostats and Guiding Telescope.

13. The telescope tube was laid approximately horizontal, in the azimuth 10° south of west, and pointed to the 16-inch cœlostat used in the 1896, 1898, 1900, and 1901 expeditions (in the last case by Mr. Newall). The 13-inch objective practically used up the whole mirror and it was thus not possible to point any other telescopes to the same mirror. But we had in our possession at Oxford a 12-inch mirror mounted in horizontal Y's, so that it could reflect any object on the meridian into a fixed direction, according to the scheme for a photographic transit-circle,* which other work has hitherto prevented our carrying out.

It was found that by tilting the base plate to the latitude of Aswan, bringing the axis into the same line as that of the 16-inch cœlostat, and joining up the two instruments, the clock could be made to drive them both, especially when a string was wrapped round a wheel on the axis of the 12-inch, and led over a pulley to a weight which thus constantly urged the

* 'Monthly Notices R. A. S.,' vol. 57, p. 349.

mirrors round and took up a great part of the work that would otherwise have fallen on the clock.

14. *Guiding.*—But we were naturally anxious as to the performance of the clock, which was thus given a double responsibility, and it was determined to watch its performance during totality, just as a guiding telescope is used in taking a star photograph. Even without the additional reason of the present instance, experience has suggested such a course. There may be a sudden fall of temperature at an eclipse which may disturb the rate of a clock, however carefully adjusted previously, or accidents of other kinds might happen. The first-rate importance of good clock driving was clearly pointed out by Professor Schuster in his report on the 1886 eclipse,* and has several times been brought before the Joint Permanent Committee. It was decided at one time to have electrical control for the eclipse clocks, but there are obvious difficulties in such a course and they have hitherto prevented its being adopted. It was determined therefore to make an experiment on guiding by hand.

15. If a refracting telescope is pointed to the cœlostat, the observer at the eye end is at a distance from it. He can use the slow motion, which is worked by a cord, to correct any defect in driving; but this method is unsatisfactory in many ways, the pull of the cord being liable to set up tremors in the instrument. It was preferred to use a reflecting telescope, which brings the observer back near the clock, and to adjust the clock rate if necessary. It was found that in the 10 minutes before totality the driving sensibly altered, but the rate was readily adjusted. During totality the alteration of rate continued, but there was no difficulty in continuing the adjustment also. The result of the experiment suggests that this operation of watching the clock throughout totality should be an essential part of an eclipse programme. Even if the clock goes well, it is a satisfaction to know it; and, so far as we know, clocks often do not go well. The change in rate at Aswan can scarcely be ascribed to the fall of temperature, which was slight, unless we include the effects of radiation, which were, of course, enormously reduced. To the body it became distinctly cooler, as when one walks from the sun into the shade; and, though the carefully-shaded thermometer showed little or no trace, it is conceivable that the clock, which was only roughly protected, may have been affected like the body rather than like the thermometer. At any rate, one other clock, at least, also felt the change, that used by Dr. Okoulitsch, of Pulkowa. The possibility of change had been mentioned to him, and he looked for it carefully, with the result that he found that the rate, which had been sensibly constant before, began

* See 'Phil. Trans.,' 1889.

to change during the partial phase, and was still changing a few minutes before totality, when he had to leave it.

16. As regards the actual arrangement adopted: a small plane mirror, kindly lent by Mrs. McClean, was attached to the axis of the 16-inch cœlostat at the north end, making in all three mirrors on the same axis. The rays were thrown on to a 10-inch concave mirror of 78 inches focus, mounted on a heavy wooden stand which could readily be adjusted on the sandy ground. They were focussed on the film of a 10-inch × 8-inch photographic plate (a trial star plate—no longer wanted), mounted in a strong wooden support. A large field was thus visible, of which any point could be selected for examination with an eye-piece held on a long rod by hand. Lines were scored with a knife freely on the film for reference marks, as it was not known where a prominence would come on the plate. Before totality, the limb of the crescent was watched. During totality, no difficulty was found in picking up a prominence to watch for the first minute, and, before it had disappeared, the amount of slight acceleration to give to the clock had practically been settled. The rate was verified by means of the reappearing prominences towards the end of totality. There was some interruption midway, owing to the accidental obstruction of the reflected beam by the operator who was exposing at the 13-inch objective. Better arrangements to avoid this risk, could doubtless be made on another occasion.

The Polariscopic Cameras.

17. *Attempts with Iceland Spar Prism.*—For the 1896 eclipse a method was devised of obtaining two images of the corona in lights polarised in directions at right angles, with a prism of Iceland spar.* The 1896 eclipse was cloudy; but the same apparatus was taken to India in 1898 and successfully used, though the pictures were on a very small scale. To obtain a larger scale, new lenses were used in Algiers in 1900, and there was revealed some defect in the prism, of the nature of an internal reflection. Before the 1901 eclipse it was sent to the makers (Messrs. Ph. Pellin, of Paris) for examination, and returned by them as satisfactory; but Mr. Newall, who had kindly offered to take out the apparatus to Sumatra, found preliminary trials so unsatisfactory that he did not use it. On preparing for the present eclipse, attempts were first made to get a larger and better prism, but failed, owing to some not altogether intelligible difficulties in the supply of Iceland spar. It was then determined to do the best possible with the existing prism, which was thoroughly tried at Oxford and then again sent to the makers with an explanation of the defect. It was not received back from them in time to

* 'Roy. Soc. Proc.,' vol. 67, p. 95.

make further trials at Oxford, but was taken out to Aswan and examined there, when it was seen at once that the defect was not cured, there being a faint spurious third image in addition to the two which should appear. Hence this method was abandoned, owing to the practical difficulty of getting a satisfactory prism.

18. *Horizontal Reflection Apparatus.*—Meanwhile it had been determined to try in addition the method suggested by Professor Schuster, of photographing the corona after reflection from a plane glass surface at the polarising angle. Some fine glass prisms* were kindly lent by Mrs. McClean from the apparatus belonging to her late husband. One of these, $9\frac{1}{2}$ inches × 7 inches and of angle 13°, was placed in front of a photographic doublet of $5\frac{1}{2}$ inches aperture and 30 inches focus; stopped down for three plates to $3\frac{3}{4}$ inches ($f/8$) and for three other plates to $1\frac{7}{8}$ inches ($f/16$). The stopping-down was done to make quite sure that the aperture was filled with light from the prism, which at an oblique incidence of 60° had only an effective width of $4\frac{3}{4}$ inches. The prism was set by means of the Iceland spar prism, the defect in which did not matter for this purpose. A small hole in the centre of the telescope field was illuminated by a candle, and the emergent beam, after reflection from the glass prism, was examined with the Iceland spar, to see if one of the images could be made to disappear by rotation. It was found that the prism could be set very exactly to the right position in this way. The angle was measured and recorded after the eclipse by an Abney level, turning the prism slope into a vertical plane and measuring the relative slope of prism and telescope tube, and found to be $57°{\cdot}0 \pm 0°{\cdot}2$. The above apparatus was used in a horizontal plane, and flexure may have slightly altered the angle on turning it round, though the mounting was firm, but the accuracy obtained is probably sufficient. In suggesting the method Professor Schuster pointed out that for a range of several degrees near the polarising angle the polarisation is very nearly complete, indeed, but for this, the method would scarcely be applicable to an object like the corona, which extends for more than a degree.

19. *Vertical Plane Apparatus.*—The apparatus mentioned in the last paragraph, used up a considerable part of the 12-inch mirror, but there was plenty of room for the Iceland spar apparatus which was prepared for use if the prism could be got right. On finding the Iceland spar prism still defective it was decided to use another reflecting prism in a vertical plane. But no telescope of the size of that already mentioned was available. After some troublesome experiments in fitting in apparatus, which comes in awkward

* Prisms were used in preference to plates to avoid the reflection from the other surface, which, in the case of a prism, is thrown in a totally different direction.

places with these oblique angles and is apt to cut off the incident beam from the cœlostat mirrors, especially with a low sun such as we had for the eclipse, an arrangement was arrived at by which a small camera of 2 inches aperture and 9½ inches focus, for which, however, only two plate-holders were available, was pointed to a prism face of 10 inches × 4 inches also kindly lent by Mrs. McClean, the plane of reflection being vertical and the angle (set as before by means of the Iceland spar prism) being found to be 61°·4. The measurements could in this case be made with the apparatus *in situ.* The exact angles for complete polarisation will be determined in the laboratory when the instruments are delivered again at Oxford, for comparison with these measurements. Attention may be drawn to the great convenience of the Abney level for measures of this kind. An accuracy of 10′ is readily attainable with this handy instrument.

Operations with the Instruments described in §§ 7 to 19.

20. The programme prepared for totality, and substantially carried out, was as follows:—

13-inch Astrographic Objective for Photographs of the Corona on 10-inch × 8-inch Plates.

Slide.	Exposure.	Plate.	Remarks.
	sec.		
1	1	Rocket	
2	10	Double-coated Rocket	
3	20	Green sensitive.........	With green colour screen.
4	5	Rocket	
5	11	Photo mechanical	
6	2	Rocket	Not exposed.

Mr. Bellamy put in the slides and Mr. Dickinson exposed at the objective. Mr. Ball received and wrapped up the plates as they were exposed.

21. Between Nos. 3 and 4 an enlargement was attempted by Mr. Kearney, with an ordinary enlarging lens of 13-inch focus placed behind the primary focus so as to give an enlargement of five diameters. But it was not sufficiently realised until too late how large the camera lens should be in order to take in the cones of rays from the 13-inch objective properly, and it was seen at the last moment that the result would be a failure. That it was not realised earlier was, of course, regrettable; but it is some excuse for the error that the trial of the camera was necessarily delayed by the difficulties of dealing with woodwork.

All the plates were developed by Mr. Kearney in the rooms of the Survey Department, Cairo, with pyro-soda.

Standard squares were printed as follows:—On plate No. 1 itself, after exposure to standard candle at 6 feet for 1 minute. On a separate Rocket plate, developed in the same dish with No. 4, to candle at 6 feet for 1 minute and also for 10 minutes. On a separate Green-sensitive plate, developed in the same dish with No. 3, to a candle at 6 feet for 1 minute direct, and for 3 minutes through the same green screen as was used with Plate 3 (the latter exposure near the label: light meeting squares, screen, plate, in that order).

22. Reflection Apparatus in Horizontal Plane.

Slide.	Aperture.	Exposure.	Plate.	Remarks.
	in.	sec.		
1	$3\frac{3}{4}$	1	Rocket	
2	$3\frac{3}{4}$	5	,,	
3	$3\frac{3}{4}$	25	,,	
4	$1\frac{7}{8}$	5	,,	
5	$1\frac{7}{8}$	25	,,	Shaken.
6	$1\frac{7}{8}$	1	,,	

Captain Lyons put in the slides; Mr. Bennett exposed at the objective and changed the stop between Nos. 3 and 4. He gave me the note about No. 5 immediately after totality. All the plates were developed with pyro-soda by Mr. Kearney at the Survey Department, except No. 2, which was developed by him at Aswan. Standard squares were printed on No. 2, 20 seconds to candle at 6 feet.

23. Reflection Apparatus in Vertical Plane.

Slide.	Aperture.	Exposure.	Plate.
	in.	sec.	
1	2	5	Rocket.
2	2	25	,,

Mr. Herbert took entire charge of this instrument. Mr. Kearney developed No. 1 at Aswan and No. 2 in Cairo, both with pyro-soda.

Standard squares were impressed on a separate plate and developed in the same dish with No. 1: 60 seconds to candle at 6 feet.

Exposures to Sky Generally.

24. Two small open tubes of 1-inch aperture and 3-inch length were exposed for 10 seconds, one to the zenith and one to the corona generally, for

comparison with similar exposures made on moonlight nights to the zenith and moon. The exposures during totality were made by Dr. W. M. Aders. Standard squares were always exposed to a candle at 6 feet and developed in the same dish with such plates.

The Goerz Lens.

25. At Mr. Maunder's request we took charge of a beautiful lens by Goerz for rapid exposures without clock driving. The lens was focussed on stars and then handed over to Mr. R. T. Günther, who took complete charge of it. His report is as follows :—

"August 30, 1905.

"Eclipse Station in Sheik's Tomb on top of hill overlooking Elephantine "Island.

Six plates exposed to Image by Goerz lens.

No.	3.	Slow.	4 secs. after commencement of totality.
„	4.	Medium.	
„	5.	Rapid.	
„	6.	Rapid.	
„	7.	Slow.	
„	8.	Medium.	25 sec. before cessation of totality.

Note.—No. 5 may not be exposed at all, and No. 6 may be badly fogged.

"Average exposure of each plate was half a second.

"*Impressions.*—Owing to the extreme brightness of the corona, which rather dazzled* me, the intensity of illumination of the landscape appeared not to alter as much as I had been led to expect by reading of books. It is for this reason that I was not able to detect any well-marked shadow travel across the landscape, although I distinctly believed that at one instant the western landscape was dimmer than the eastern. There appeared to be no *sharp* delimitation between the two illuminated areas. No ripple waves of light were seen on a sandy slope to W.N.W. of observer, but as he had other things to think about, this slope did not have his undivided attention.

"R. T. Günther."

On developing the plates, Mr. Günther's fears with regard to 5 and 6 were found to be too well-founded. By stopping the development of the fogged plate it was possible to save a faint image of the prominences and inner corona.

* Mr. Günther covered up his eyes for some minutes before totality.

The Reflector of 120*-feet focus.*

26. No satisfactory photographs of the corona were obtained with this instrument owing to several contributing causes. One of these causes alone, however, was sufficient in itself to completely spoil the results—viz., on developing the plates they were found to be strongly marked by the shape and grain of the woodwork of the plate-holder. The reason of this action of the woodwork is still under investigation, but it has already been found that it does not depend on any exposure to light. On discovering the curious action, the experiment was tried of putting a plate into the holder and locking it up in the current of the dark-room at the Survey Department for a day, so that it never saw any light at all. On development, strong markings of the woodwork of precisely the same character were found. Attempts will be made to get at the bottom of this matter, which is liable to seriously affect photographic work: to see, for instance, whether a high temperature is an important factor; but this investigation must be given separately. So far as it is possible to make out what there is on the plates in addition to these markings, it seems that the difficulties of mounting the telescope were not completely conquered. That very little appears beyond the prominences may be partly accounted for by the presence of the woodwork image and the necessity of stopping the development in consequence; but the definition is not good, and in one exposure the instrument was shaken.

It is, of course, a great disappointment to get practically no return for a good deal of work; and it only remains to record the facts leading up to it.

27. It should be remembered throughout that the eclipse work was not the main object of Mr. Reynolds' visit to Egypt. His main purpose, as already mentioned in § 1 (*c*), was to superintend the erection at the Helwan Observatory of a reflecting telescope, the 30-inch mirror of which was made by the late Dr. Common, and the mounting designed and constructed by Mr. Reynolds himself and presented to the Helwan Observatory. The eclipse of August 30 had an obvious influence on the time of his visit to Egypt, but by fixing a definite date for the delivery of the instrument, it seriously added to the amount of work to be done in the last six months in the scanty leisure of a busy man. This being premised, the following brief account of the enterprise will be better understood.

28. The project of taking out the long focus reflector was first seriously considered in the autumn of 1904. The instrument was mounted in the garden of the house at Birmingham where Mr. Reynolds resides (his father's house). The concave was placed horizontally due south of the cœlostat mirror, and in this position neither sun nor moon could of course be seen,

using the cœlostat in the ordinary way. But Mr. Reynolds chiefly used the telescope for gazing purposes, and he had mounted the cœlostat mirror on pivots so that it could be tilted at any angle to the polar axis. Hence the sun or moon could be thrown on the concave, and focussed near the cœlostat at any time, though the image was no longer stationary. Using the instrument in this way, Mr. Reynolds had found out a good deal about its behaviour; he knew that good definition was only obtainable occasionally, but that at times it was obtainable; also that the instrument was liable to changes of focus.

29. On his kind invitation I paid two visits to Birmingham to see the instrument. On the first occasion, unfortunately, a dense fog came on and nothing could be done. On the second the moon was full and bright, but it was a frosty night. The focus was found to be fully 1 foot within its previous record, and the definition was not good. On discussing the matter it was decided that the prospects of success were perhaps too slender to justify undertaking the enterprise as it stood, but Mr. Reynolds determined to make a concave of shorter focus (about 80 feet) to see whether good definition could not be secured more regularly, and he further visited the works of Messrs. Chance and Co. to see whether some glass could not be obtained which would be less subject to temperature effects. Messrs. Chance and Co. were able to recommend such a glass, and a disc was ordered. But now the work on the 30-inch needed attention, and it was found impossible to provide this new disc in time. Accordingly it was decided to give up the idea of a long focus telescope and to use the Helwan 30-inch mirror in conjunction with the cœlostat, enlarging the primary image with a 6-inch enlarging camera.

30. On July 17, however, Mr. Reynolds, in writing about this plan, mentioned incidentally that he had had a very fine view of a sun-spot the day before with the 120-foot; that the "definition at instants was certainly very good." In consequence I suggested that perhaps after all it might be well to revert to the original experiment. It was the one for which a sub-committee had been specially appointed, and an opportunity of trying it was not likely to recur for some time. Moreover, although success was problematical, there seemed a fair chance of it, and if it could be obtained the gain might be great. Mr. Reynolds concurred and it was determined to take the risk. Time was, of course, running short, but it was arranged that I should select a site in advance and build the piers, and that Mr. Reynolds should get ready the instrument to bring out with him later.

31. On arriving at Aswan, a site for the instrument was originally

selected in the hotel grounds on Elephantine Island, but as already mentioned in § 4, a new site was chosen at the railway station when it was found how little time was available. Here a pier 8 feet × 5 feet was erected on the railway platform for the cœlostat and camera; and 120 feet away, on the enclosure boundary, a wall several feet in length was built for the concave, to give a range both in focal length and in azimuth of the reflected ray. The azimuth for horizontal reflection was 10° south of west, but this took the rays rather close to a palm tree, and it was felt that it might be desirable to choose an azimuth further north with a corresponding depression of the ray.

The instrument arrived at Aswan late on August 26. Early next morning the work of mounting the cœlostat was commenced; and it was practically completed and the necessary adjustments made by the afternoon. The sun's rays were then flashed across towards the other pier to select the exact place for the concave, and immediately a serious difficulty presented itself. It has been already mentioned that the plane mirror was provided with an additional movement, being mounted on an axis perpendicular to the polar axis. One of the bosses at the end of this axis came into contact with the backbone of the cœlostat and prevented the mirror from assuming a position near the vertical. The sun's altitude at eclipse was 24°, and to reflect its rays horizontally the mirror must be inclined only 12° to the vertical, whereas the minimum possible inclination was greater. How much greater it was not easy to say, for it was already past the time of the eclipse; and only a rough estimate could be formed, from observation of the place where the rays crossed a certain palm tree, at a given time, of the direction they would take at eclipse time. At night attempts were made to use Altair, the declination of which was within 30′ of that of the sun at eclipse, to estimate the position more closely; but there is some difficulty in identifying stars from 120 feet away in a mirror which subtends an angle of only about a degree, and it was felt that the procedure could not be trusted. Next morning it was found by observing the time taken by the sun's rays to rise from the horizontal position to that marked on the palm tree that the amount of alteration required was not very large, and it was hoped that by chipping away the cast-iron backbone of the cœlostat, enough might be done.

A segment of about ½ inch in depth was chipped away during the day, shelters erected for the two mirrors, and some necessary repairs made on the clock, which had been broken in transit; and at eclipse time the reflected rays were anxiously observed. It was seen that enough had not been done; the mirror came to a stop just before totality time. It was

then decided to tilt over the whole stand, by wedging up from the east. This involved readjustment of the instrument and a certain loss of stability; but there seemed to be no choice. To restore stability as far as possible sand and earth were packed all round the base. This was not completed till the morning of Tuesday, August 29. Meanwhile it had become clear that the pier for the concave must be raised, and all idea of a depressed ray, with better clearance of the palm tree, given up. Instead of building up bricks, which would have taken valuable time, a pile of steel railway sleepers, kindly lent by the authorities, was made, and the concave mounted upon them.

Having settled the direction of the ray, a tube for it was made by mounting 10 stout square wooden frames on posts, running wire through staples in the frames, and stretching canvas along the wire. This work took almost every available minute up to the time of eclipse, but all was actually ready in time. The ground under the tube had, as already mentioned, been thoroughly soaked by the local fire-brigade, and radiation effects thereby considerably reduced. Against air-tremor nothing could, of course, be done; but a new and formidable source of alarm presented itself almost at the last. A powerful steamer went down the river and the whole instrument shook to the paddle wheels. Apparently the ground transmits vibrations with terrible facility. When this was realised, all idea of a long exposure was abandoned, and it was determined to confine the exposures to two or three seconds. Even then, as the event proved, shake was not avoided in one exposure.

During totality Mr. Reynolds adjusted the cœlostat so as to throw the selected portion of the sun's limb on to the plate, Mr. Keeling put in the plates and the focussing screen for adjusting them, and Mr. E. M. Dowson watched the clock, which showed irregularities in driving sometimes. It has been already mentioned that the plates, when developed, were spoiled by some action of the wooden plate-holders, which gave a strong image of the woodwork.

Solar Eclipse Expedition to Kalaa-es-Senam, Tunis.

By Professor L. BECKER, Ph.D., F.R.S.E., F.R.A.S.

(Received November 11, 1905.)

When the Court of the University of Glasgow made to me an unsolicited grant of £50 to see the eclipse, I decided to take active part in its observation, though in a small way. Together with Mr. Franklin-Adams I accepted the invitation of Mr. Andrew Crookston, Glasgow, to observe it at his mines at Kalaa-es-Senam, Tunis. The station lies 33 m. 30 s. east of Greenwich (from map) at a latitude of 35° 45′ 19″ as determined from pole-star observations. Its altitude above sea-level is 953 metres. It is about 40 miles from Morsott, a station on the railway from Bona to Tebessa.

I set myself the problem to determine from a series of correctly-timed photographs the law according to which the light of the corona decreases with the distance from the sun. I designed mechanisms by which 10 exposures are automatically made on one plate, the mechanisms being governed electrically by a pendulum clock. I employed two cameras, one with a Cooke triple achromatic lens of $3\frac{1}{2}$ inches aperture and 60 inches focal length, which belongs to the Glasgow spectrograph, the other with a Ross portrait lens of 2 inches aperture and 12 inches focal length. They were fed by a cœlostat of 8 inches aperture, which had been kindly lent to me by the Royal Dublin Society. In front of the two object-glasses a rotating shutter was mounted which served both cameras. The rotating shutter has four oblong apertures, 90° apart; it is rotated by clockwork driven by a spring, and its motion is governed by the armature of an electromagnet. When the armature is attracted the shutter rotates through 45°, bringing an opening opposite the object-glasses, and when it is released it turns again 45°, when the shutter shuts off the light. The contacts are made by a pendulum clock, and they are so devised that make or break can occur only when the pendulum is at or near its position of rest. I arranged for five exposures of 1 second duration, and five exposures lasting respectively 3, 9, 20, 46, and 89 seconds. Their actual durations are 0·86, 0·82, 0·80, 0·82, 0·87, 2·82, 9·02, 20·84, 45·91, and 89·04 seconds, as determined automatically on the chronograph. At the first four exposures of 1 second, different screens, each with 13 holes, are in front of the object-glasses. At the first exposure the screen leaves about one-sixteenth of the object-

glasses free, at the second one-eighth, at the third one-fourth, and at the fourth one-half. These screens are geared to the clockwork which rotates the shutter.

The plate-holder of the Cooke camera is 17 × 3 inches, it slides lengthways inside a metal box 32 × 4 inches. It is moved by rack and pinion, the rack being attached to the plate-holder and the bearings of the axis of the pinion being fixed to the box. Spring-driven clockwork communicates its motion by means of a shaft to the pinion. The clockwork is governed by the armature of an electro-magnet; when the armature is attracted the plate-holder moves 1 inch onwards, and when it is released it moves another inch. The necessary contacts are made by the pendulum clock. The same mechanism actuates on the plate-holder of the small camera, only the steps are correspondingly smaller. I arranged the contacts in such a way that for the first four exposures the plate moved one step onwards, for all the others two steps, and when the plate had been slid along I allowed 2 seconds for the camera to settle before the next exposure was made. Of the 206 seconds for which I made provision, 173 seconds are occupied by the exposures, 15 seconds are taken up by changing of plates, and 18 seconds are lost. Owing to reasons which I need not explain here, I did not unpack the boxes until August 26. Unfortunately, I found the automatic apparatus damaged, though it had been carefully packed, but not sufficiently for a journey on a road like that from Morsott to Kalaa. I repaired the damage as well as I could on the 27th, and adjusted the cœlostat on the 28th. On the 29th a southern gale made all observations impossible, and, in fact, all apparatus except the cœlostat had to be dismounted and taken indoors. On the morning of the eclipse they were re-erected and tried, not, however, with the plate-holders, because I was afraid that the plates, which had been placed inside the plate-holders, would be spoiled by the heat. When the signal of the beginning of totality was given, I set the pendulum in motion, and all went right until the fifth exposure (1 second) was finished, then the plates advanced only one step instead of two, and the same took place after the next exposure (9 seconds); between the seventh exposure, of 3 seconds, and the eighth exposure, of 89 seconds, the propelling mechanism failed to move the plates. Before the last two exposures, of 21 seconds and 46 seconds, the plates were moved on their proper amount. In consequence, the two exposures of 89 seconds and 3 seconds are superposed, giving one of 92 seconds, and the images belonging to the fifth, sixth, and seventh exposures are at half the distance from one another that I had meant them to be. The irregularity was caused by friction of some damaged parts of the mechanisms. I developed the plates the same night, the two plates of the

Cooke camera, which are cut from the same full plate, and the small plate of the Ross camera being put in the same developing tray for seven minutes. The result is very satisfactory. Each camera has furnished nine pictures of the corona instead of the ten arranged for. Eight photos by the Cooke camera are fine from the pictorial point of view, especially the five of 1 second exposure and that of 21 seconds. The focus appears to be correct, and the cœlostat has performed excellently. For the 92 seconds exposure the photographic plate was too small. It will be difficult to reproduce the negatives on one plate on account of the densities of the background of the various negatives, which increase with the exposure. There is direct evidence that the light which illuminated the field came from the cœlostat mirror, and though the sky was unexpectedly bright, I think it must be attributed to coronal light diffusely reflected by the large whitish dust particles which were always settling on the mirror. I might have suppressed this background by judicious developing had it not been my aim to introduce nothing that I could not exactly reproduce at home. The long-exposure photographs obtained with the Ross portrait camera are over-exposed, that of 90 seconds to such an extent that the corona close to the moon's limb and the protuberances are reversed. Since my return home I have measured the plates and reduced the measurements. Experiments will be undertaken without delay from which I can derive the relation which connects time of exposure and intensity of radiation for the same degree of blackness on the photographic film. I shall employ the same photographic plates and develop them in the same way as the eclipse photographs.

Address delivered by the President, Sir William Huggins, K.C.B., O.M., F.R.S., at the Anniversary Meeting on November 30, 1905.

Since the last Anniversary the Society has lost by death twelve Fellows and four Foreign Members.

The deceased Fellows are:—

Dr. W. T. Blanford, C.I.E., born Oct. 7, 1832, died June 23, 1905.
Sir Lowthian Bell, born Feb. 15, 1816, died Dec. 20, 1904, aged 88.
G. B. Buckton, born May 24, 1818, died Sept. 25, 1905, aged 88.
Professor G. B. Howes, born Sept. 7, 1853, died Feb. 4, 1905, aged 51.
Capt. F. W. Hutton, born Nov. 16, 1836, died Oct. 27, 1905, aged 68.
James Mansergh, born April, 1834, died June 15, 1905, aged 71.
H. B. Medlicott, born Aug. 3, 1829, died April 6, 1905, aged 76.
Sir Erasmus Ommanney, born May 22, 1814, died Dec. 21, 1904, aged 90.
Sir B. Samuelson, born Nov. 22, 1820, died May 10, 1905, aged 84.
Sir John Burdon Sanderson, Bart., born Dec., 1828, died Nov. 23, 1905, aged 77.
Sir William Wharton, born March 2, 1843, died Sept. 29, 1905.
Sir Charles Wilson, born March 14, 1836, died Oct. 25, 1905.

The Foreign Members are:—

O. W. von Struve.
P. Tacchini.
Baron von Richthofen.
Albert von Kölliker.

Memorial Notices of the Fellows and Foreign Members who have been taken from us by death during the past year will appear in due course in the Obituary Notices. Of some of them only, on this occasion, will time permit me to give expression on your behalf, to a few words of appreciation of their work, and of deep sorrow at their loss.

Not among the Fellows only, or alone in this country, but throughout the scientific world, the news of the unexpected death of our Fellow, and recent Vice-President, Dr. William Thomas Blanford, was received with deep regret and sorrow. Not only had a distinguished worker in science fallen out, there was lost to us a gentle, kindly friend, who had gained the affectionate regard of all those who had the privilege of having been personally acquainted with him. Only a few weeks before his death, he had been asked by the Council to write an Obituary Notice of our late Fellow, Mr. Medlicott, his

old friend and colleague, and collaborator with him in the classic work, 'The Manual of the Geology of India,' published in 1879. Before the printer's proofs reached him, Dr. Blanford himself had passed from the ranks of the living. Dr. Blanford was distinguished as a zoologist as well as a geologist. From the time of his appointment to the Geological Survey of India in 1855, on the completion of a very successful course of study at the Royal School of Mines, and at the Mining Academy at Freiberg, to his retirement in 1882, by the publication of a series of works, and by untiring original observations, he greatly enriched our knowledge of the geology and zoology of that country. Besides his published works on the geology and fauna of India, he has contributed important papers and addresses, which are distinguished by great scientific insight and a masterly grasp of the subjects to which he had devoted his life. His high and kindly qualities were fully recognised. He received the distinction of the Companionship of the Order of the Indian Empire, and was awarded medals by the Royal Society and the Geological Society. He was elected into our Society in 1874, and was for many years Treasurer of the Geological Society.

To Mr. Medlicott belongs the honour of having largely contributed to the laying of the foundations of our knowledge of the geology of India. Born at Loughrea, county Galway, after taking his degree at Trinity College, Dublin he entered the Geological Survey, first of Ireland, and then of England. After holding for some ten years the Professorship of Geology in the Roorkee College of Engineering, he joined the Geological Survey of India. In 1876 he became Superintendent (a title subsequently changed to Director) of the Survey. A man of great activity, high courage, and of most liberal policy in regard to his subordinates, his appointment to the head of the Survey gave a powerful impulse to active work in all its departments. He contributed a large part of 'The Manual of the Geology of India,' written, as already mentioned, in collaboration with Dr. Blanford, and published by the Government of India in 1879. Elected into our Society in 1877, he received the Wollaston Medal of the Geological Society in 1888. He was a Fellow of the Calcutta University, and for three years President of the Asiatic Society of Bengal. For his distinguished courage in saving the lives of a family at the time of the Indian Mutiny he was awarded a military medal.

Mourned by a very wide circle of friends, the distinguished and veteran ironmaster and metallurgist, Sir Isaac Lowthian Bell, passed from us at the ripe age of 88. After a course of successful study in physical science in Edinburgh, and then at Paris, he entered the Walker Ironworks near Newcastle. About ten years later he became connected with the chemical works

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at Washington, in North Durham. Under his management the works were greatly enlarged, and the manufacture of oxy-chloride of lead was introduced to take the place of white lead. Bell will be chiefly remembered in connection with the development of the Cleveland iron industry, in which, by the establishment of works at Port Clarence, on the north bank of the Tees, he played a very important part. Especially the firm was active in prosecuting the technical experiments by which the processes have been devised enabling Cleveland ores to compete as raw material for the production of iron and steel.

In 1865, Bell contributed to the British Association a paper on the manufacture of iron in connection with the Northumberland and Durham coal-fields. He was the author of numerous papers on the chemistry of iron and steel, which were collected and published in a thick volume with the title 'The Chemical Phenomena of Iron Smelting.' He was also the author of a work on the 'Principles of the Mauufacture of Iron and Steel.' He was one of the founders of the Iron and Steel Institute, of which he filled the office of President in 1873 to 1875. He was elected into our Society in 1874, and received honorary distinctions in America and on the Continent. In 1876 he was awarded the Albert medal by the Society of Arts, and in 1885 the honour of a baronetcy was conferred upon him, and later, in 1893, the degree of LL.D. by the University of Edinburgh.

It is with deep sorrow that I put on record the death of a member of the present Council, a Fellow widely known and respected and beloved by every one who knew him. Rear-Admiral Sir William Wharton may be said to have fallen a martyr to science, having contracted pneumonia and enteric fever on his visit to the Cape, to preside over the Geographical Section of the British Association. He entered the Navy in 1857, and on passing his examination five years later, gained the Beaufort prize for distinction in mathematics, astronomy and navigation. In 1872, he became Commander, and was appointed to the surveying ship "Shearwater," for service in the Mediterranean and the East Coast of Africa. In this work he distinguished himself by his investigation of the surface and undercurrents of the Bosphorus, setting at rest the controversies respecting the constant flow of water from the Black Sea to the Sea of Marmora. The following year he commenced, in the "Fawn," the survey of the Red Sea and the East Coast of Africa, finishing with the Sea of Marmora. On his return, he published his book, 'Hydrographical Surveying.' He was promoted to the rank of Captain in 1880, and after surveying work in the River Plata and the Straits of Magellan, he succeeded Sir Frederick Evans as Hydrographer to the Admiralty, a post which he held for 20 years with distinction to himself, and with great

advantage to the navies of the world. Last year failing health determined him to resign his position as Hydrographer. Sir William Wharton was a man of sound judgment, wide scientific attainments, and great capacity for work. He was elected a Fellow of our Society in 1886, and his official work was recognised by the distinction of K.C.B. conferred upon him at Queen Victoria's Diamond Jubilee. He was an active Fellow of the Royal Astronomical Society, and of the Royal Geographical Society.

To the deep grief of a large circle of friends, George Bowdler Buckton passed away at the age of 88, leaving behind him a noble example of unwearied devotion to the successful prosecution of scientific work, notwithstanding great physical infirmity due to an accident in his fifth year. In early life his attention was given chiefly to physical science; he was assistant to Professor Hofmann at the Royal College of Chemistry, and during the 20 years between 1845 and 1865 he published several important papers on chemical subjects, in connection with which he was elected a Fellow of our Society in 1857. His life work, however, was done in Natural Science, when, returning to the early love of his boyhood, he devoted himself to original research in Entomology, relating chiefly to the order Homoptera. From 1876 to the present year he published a series of important entomological monographs. The numerous plates, many of them coloured, by which the monographs were illustrated, were drawn, and the pattern plates coloured, by his own hands; some plates were even lithographed by himself. Mr. Buckton was a man of wide culture, a musician as well as an artist, taking an unflagging interest in every question affecting mankind, and an active part in local affairs. He was a member of many scientific societies at home and abroad.

It is with much regret that I have to record the death of Admiral Sir Erasmus Ommanney, a man full of years, who was personally known to many of the Fellows, and was a frequent attendant at our meetings. Born in 1814, he early entered the Navy, in which he rendered distinguished services to his country. He was present as a midshipman at the battle of Navarino in 1827. As lieutenant he was appointed to a small frigate, which, under the command of Sir James Clark Ross, proceeded to the dangerous expedition of rescuing a number of whalers reported to be caught in the ice in Baffin's Bay. In 1850–51 he commanded the "Assistance" in the Arctic Search Expedition, and was the actual discoverer of the first traces of Sir John Franklin. He directed an extensive system of sledge journeys, by which the coast of Prince of Wales Land was laid down. Sir Erasmus Ommanney was elected into the Society in 1868, was knighted in 1877, and received the further honour in 1902, of K.C.B. In 1890 he received from the King of Greece, the Cross of Grand Commander of the Order of the Saviour.

The death of Mr. James Mansergh, elected into the Society so lately as 1901, deprives the world of an engineer of very high authority on the questions of water supply and the disposal of sewage. After his majority in 1851 he carried through very successfully the Don Pedro II Railway, connecting Rio de Janeiro with the interior, 200 miles of which had to be constructed mostly through the virgin forest. He returned after three and a-half years, leaving two of his comrades in their graves and a third invalided home. After his return, he laid out the first sewage farm in England, and afterwards was entrusted with the heavy and difficult main sewerage contract at West Ham. Mr. Mansergh's advice and assistance were sought not only for many great works in this country, including the gigantic water scheme for Birmingham, but also on the Continent of Europe, in America, and in Australia, where he advised as to the great work for the sewerage of the district of Melbourne. Owing to his wide experience and his mature judgment, Mansergh was frequently called upon to take the responsible post of arbitrator or umpire in connection with water and gas undertakings. He was elected to the office of President of the Institution of Civil Engineers in 1900.

Professor Howes, who passed away prematurely at the age of 51, to the great sorrow of his many friends, showed, even as a schoolboy, his innate love of science by his preparation during his spare time of microscopical slides and dissections. He received an appointment at South Kensington, and made, under Huxley's direction, large, coloured, anatomical drawings of various animals for the Biological Laboratory. Subsequently he was appointed Assistant Demonstrator, and, a little later, Chief Demonstrator, and, when Huxley partially retired in 1885, Assistant Professor. Ten years later, when the Chair of Biology was subdivided, Howes became Professor of Zoology. His scientific publications are numerous, consisting of papers and addresses; among the latter should be mentioned his address as President of the Zoological Section of the British Association at its meeting at Belfast in 1902. His 'Atlas,' of which two editions have appeared, is well known, and he brought out revised and extended editions of Huxley and Martin's 'Elementary Biology.' His original work deals mainly with vertebrate comparative anatomy. Howes' power of work was remarkable, and his knowledge and memory of detail and of current biological literature almost unique, though in his lectures and his writings he always kept before him the main issues, and was always clear and stimulating. Generous and unselfish in all he did, his loss is deeply felt by a wide circle of friends. He was himself a remarkable example of his own belief that "higher ambition than that of adding to the sum of knowledge no man can have: wealth,

influence, position, all fade before it; but we must die for it if our work is to live after us."

The oldest in date of election of our Foreign Members, Professor Albert von Kölliker, has passed away, at the age of 88. In him we mourn one of the founders of modern systematic histology, and one of the most distinguished investigators and teachers in the departments of embryology and comparative anatomy. His memoirs and writings are too numerous to mention here. In 1847, Albert von Kölliker was appointed to the Chairs of Physiology and Comparative Anatomy in the University of Würzburg, and shortly afterwards joined Siebold in founding the 'Zeitschrift für wissenschaftliche Zoologie.' He is the author of many important works on microscopic and comparative anatomy, and on embryology, all of which have contributed largely to the advancement of those sciences, and of which most have passed through several editions.

In 1896, as a recognition of his brilliant scientific services, he was nominated Knight of the Order *pour le mérite*. The Royal Society, in 1897, awarded him the Copley Medal, the highest honour which it has to bestow.

At the last Anniversary I occupied a few minutes in bringing to your remembrance some of the more important occasions on which the Society in the past had initiated, supported, or given advice about scientific questions in connection with the State; and, at the same time, I called attention to the large number of responsible public duties which to-day rest permanently upon it, and by which, either through departments of the State or through other public bodies, the Society makes its influence felt strongly for the good of the country.

To-day I wish to speak of the profound influence which the discoveries of science, in great part the work of Fellows of this Society, have had upon the general life and thought of the world, especially during the last fifty years.

The untold material benefits which science has conferred upon civilised mankind are too familiar to need mention; they are always with us, from the world's news upon our breakfast table to our sun-bright evenings. There are, however, other benefits more subtle and less obvious, but not less real and certainly not of less price—the wider range of thought and the greater intellectual freedom which have followed upon modern scientific discovery.

I am justified, surely, in saying that the average way of thinking on all subjects has been as much altered and elevated by the researches and writings of men of science as have been the common conditions of living. The contrast in what and how we think to-day, as compared with the day

on which the Society received its Charter, is as great as it is in how we live and travel.

The changes which have taken place in the scope and mode of national thought, especially during the last fifty years, have been brought about mainly in two ways: by a breaking down of inherited prejudices and of traditional opinions through the results of scientific discovery; and, secondly, by the freer and more direct methods of thinking which have followed from the experimental study of nature.

The Royal Society was itself a chief practical outcome of a new spirit, which, during the generation preceding its foundation, had arisen at Oxford and elsewhere, and was stirring into life the dry bones of a rigid and antiquated philosophy. Scholasticism, already in decay, was slowly losing its hold upon the more active minds who refused to accept any longer as final the traditional hypotheses and syllogistic methods of the schools in the interpretation of natural phenomena. There was growing slowly a conviction of the necessity in the study of nature, of an appeal to Nature herself by means of direct experiment.

Of the great minds which had come into this state of mental unrest, the most original and creative was Francis Bacon, who, by the unequalled power and eloquence with which he summed up and put into a connected system, the new ideas which were in the air, gave so great an impulse to the newer mode of thinking, as rightly to have received the name of the "Father of experimental philosophy." His immediate success was due, however, in no small part, to the circumstance that the time was ripe for the great changes in the way of studying nature, which, in his writings, he so powerfully expounded and enforced.

I must pause for a moment to say how very unfortunate in this respect was the lot of his great, if not greater, namesake, Roger Bacon, the "Doctor Mirabilis," as he was properly named, who, born out of due time, exerted but little influence on contemporary thought.

Let us not forget that it was Roger Bacon, who, 300 years before the time of "large-browed Verulam," saw clearly that the study of nature could only be successfully prosecuted and advanced by means of experimental research, and so gave it the highest place as *Domina omnium scientiarum.* The reasons which he gave for his exaltation of experiment, might have been written yesterday, so modern is his standpoint. "Experimental science" he says, "has three great prerogatives over all other sciences: it verifies their conclusions by direct experiment; it discovers truths which they could never reach; and it investigates the secrets of nature, and opens to us a knowledge of the past and of the future."

To return to Francis Bacon: his philosophy was summed up in the words, *imperium hominis*, the great destiny of man as the ruler of nature; and he saw that man's rightful sovereignty over nature could only be attained through the slow and laborious acquirement of a true understanding of nature. Bacon looked upon nature as an overwhelmingly complex congeries of phenomena, and as a *filum labyrinthi* by which man might slowly find his way through its mysteries to all knowledge, he put forward and expounded in the 'Novum Organum' his new method, *spes est una in inductione vera.*

It must not be forgotten that Bacon's induction is something more than the traditional induction of the logicians, and practically became a new method, since it includes the elimination of the non-essential. It is no disparagement of the great and revolutionary work of Bacon, to acknowledge that the discoveries of science during the last two centuries and a-half have not been won by an exclusive following of his method. For example, he assigns no proper place to the use of the trained imagination in scientific experiment, though, indeed, he speaks of the procedure from one experiment to another as an art, or a learned sagacity. Further, there is in his system no sufficient appreciation of the deductive method of reasoning.

On these grounds questionings have made themselves heard, and in some quarters, rather loudly, whether Francis Bacon has a right to the high position usually accorded to him in the history of experimental science. We shall probably not go far wrong if we allow ourselves to be guided by the views of Bacon taken by his immediate intellectual successors, the great men, Boyle, Evelyn, and others, who had the chief part in founding the Royal Society. We find them reflected in the Ode to the Royal Society, composed, at the instance of Evelyn, by the contemporary poet Cowley. He likens Bacon to a modern Moses who led the chosen people to the promised land of knowledge of nature, though he himself did not enter, and only viewed it imperfectly from afar. The fine engraving by Hollar which forms the frontispiece to the large paper edition of 'Spratt's History of the Royal Society,' published in 1667, the design of which was furnished by Evelyn, contains two principal figures: the first President of the Society, Lord Brouncker, is on one side of the bust of the Royal Founder, and on the other is Bacon, with the title of *Artium Instaurator.*

If the methods and discoveries of science can exert the large influence on general thought which I have claimed for them, some explanation may be needed of the great slowness of any incoming, to an appreciable extent, of a wider and freer spirit during the first centuries of the Royal Society's existence. Two hundred years went slowly by, without any very marked

change in this respect showing itself in the intellectual attitude of the people. The public mind, on all questions which have to do with man's position in relation to nature, still slumbered on under the narcotic influence of traditions which were regarded as too sacred to be open to discussion. Still, during these 200 years, the leaven of the open mind of scientific research was silently at work, for each true student of nature became, among those about him, the source of a new and living influence. The fact was that, during all that time, there was no real mental contact, no true understanding, between the man of science and the average man of education. The mind trained to receive without questioning the teaching of traditional authority, and the mind eager to find out new truth in the spirit of the Society's motto, *Nullius in verba*, had little in common; they were even often mutually repellant. It could hardly be otherwise; there was no popular scientific Press, and in the halls of the Schools the drone of monotonous repetitions from memory of knowledge sanctioned by authority was never broken in upon by the jubilant eurekas of experiments, however simple, or of individual observation of nature.

What in the intellectual world would correspond to a thunderbolt or an earthquake was needed to awaken and transform the slumbering age—and it came. In the early years of Queen Victoria's reign the accumulated tension of scientific progress burst upon the mind, not only of the nation, but of the whole intelligent world, with a suddenness and an overwhelming force, for which the strongest material metaphors are poor and inadequate. Twice the bolt fell, and twice, in a way to which history furnishes no parallel, the opinions of mankind may be said to have been changed in a day. Changed, not on some minor points standing alone, but each time on a fundamental position which, like a key-stone, brought down with it an arch of connected beliefs resting on long-cherished ideas and prejudices. What took place was not merely the acceptance by mankind of new opinions, but complete inversions of former beliefs, involving the rejection of views which had grown sacred by long inheritance.

I need scarcely say that I am speaking of two scientific discoveries, following each other at no great interval of time, about the middle of the last century, and both due mainly to the work of Fellows of the Society. The first discovery was the evidence from geology for the great antiquity of the earth, as opposed to the all but universal belief of the time, and then evidence for the great age of man. The second discovery, of a not less revolutionary import, was the doctrine of organic evolution by the principle of natural selection, which brought about a complete change of opinion as to the position of man himself in relation to nature.

If I speak strongly it is because I lived through that period, and my recollections are still vivid of the fierce fury of the storm of opposition with which both these innovations of thought were at first assailed. It seems to me that these signal victories of new knowledge gained by experimental methods of research over views in which for generations men's minds had been fast riveted by tradition and authority, placed natural science, for the first time, in its true position, as within its own sphere the absolute authority to which all must bow. Up to that time, science had been on sufferance; welcomed, indeed, when it contributed to the supply of man's material needs, as by the steam-engine and the railroad; dallied with and sometimes smiled at, when her conclusions did not clash with what men had been taught to regard as unassailable truth; but rejected with scorn, and her prophets vilified with epithets borrowed from the darkest times of mediæval persecution, whenever, in the spirit of the Society's motto, she dared to utter words which were not in agreement with inherited beliefs. Then, to some extent, the true position of natural science was acknowledged, and she came into her own—the crown and sceptre of authority, which are her right—as, to repeat Roger Bacon's words, *Domina omnium scientiarum.*

Ever since that time, notwithstanding cavillings here and there, of which the echoes are still audible, natural science has taken a truer place in relation to the general thought of the age. Her position of supreme authority has been recognised, and each year strengthened by the unbroken series of brilliant discoveries which have distinguished the last half-century, and which have impressed themselves so much the more deeply on the public mind, because they have been lavishly accompanied by practical applications and inventions, which have increased, to an extent almost beyond words, the power, richness, and happiness of human life.

This is not the place to discuss in full how fruitful have been in all directions of human thought, and so, for the progress of mankind, the two great revolutions of opinion of which I have been speaking, especially the one that came a little later, and that will for all time be associated with the name of Charles Darwin, of which the innate vitality is so great that it has already grown into a great tree of knowledge bearing all manner of fruit. It is, indeed, true that before Darwin the idea of a continuous development, alike in the physical and biological worlds, had formed the basis of speculations in many quarters; but this conception, being contrary to current belief, had left no impression on the general mind. It was not until Darwin's works appeared that the new evidence was perceived to be overwhelming in favour of the view that man is not an independent being standing alone, but is the outcome of a general and orderly evolution. It follows from this view

that the principle of evolution must henceforth take a guiding place in the consideration of all problems relating to man, to the history of his fundamental convictions and opinions, as well as to all social and economic questions of the present and of the future.

To the open eye all the world is indeed a stage, the boards themselves having been laid by an earlier evolution, on which, through ages, the Drama of the orderly evolution of living things has been going on. Through the revelations of palæontology we can, in imagination, become spectators of the Scenes of the earlier Acts of the slow progress of events leading up to the entrance upon the stage of man himself. Then in archæology and history, as in magic mirrors, we can see re-acted the early Scenes of the final Act (which is still in progress), in which man plays the principal part. The strident brass was softened when nature's orchestra modulated into the melodic and more joyous *leit-motiv* heralding the coming on of man. In the later Scenes, Intelligence has come on to take the leading part hitherto played by Brute Force, and man has brought with him into the Drama the new characters of Pity, Mercy, and Charity.

Henceforth the dominant power in the world is brain, controlled by the emotions of the heart; and the highly-trained intelligence the chief factor of success in all departments of individual and national enterprise.

One of the most important and fruitful results of the intellectual upheaval which followed upon the two great discoveries of science, of which we have been speaking, is the almost unlimited freedom of personal belief which we enjoy to-day. The older Fellows, who, like myself, lived through that eventful time, will not have forgotten the narrow and bigoted spirit which then prevailed. Though without the name, and unsupported by the terrors of rack and stake, in fact and in deed, an inquisition was still in power. The reproach of heresy was freely used, and those who dared to think for themselves, and, exercising their private judgment, to swerve from the current opinions sanctioned by antiquity, were made to feel how heavy could be the social penalties enforced by the spirit of persecution.

Experimental science came as the liberator of men's minds, setting free from the prison house of conventional beliefs the spirits which had been lying for generations in the bonds of the dogmas of past ages. Slowly men came to acknowledge that the arbitrary authority of names, and of systems of belief, however greatly venerated, must give way when science speaks with the reasonable authority of experiment and observation. This new form of authority, to which men were coming to yield an unquestioning obedience, unlike the dogmatic teachers at whose feet they had sat, does not claim finality for its opinions. It is the distinctive glory of experimental science

that it is for ever seeking further truth in all directions, and is always ready to change its opinions into agreement with the newest knowledge, whithersoever it may lead, which it is able to wrest from nature by experiment. There are many striking recent examples, of which I will mention only the unexpected phenomena of radio-activity, and the acute earnestness of the biologist of to-day in his quest after the fundamental nature and scope of living things.

In this way, during the last half-century, under the freer conditions of general thought introduced by natural science, men gradually became accustomed to wide differences of personal opinion, and so no longer feared them; there arose slowly the spirit of modern toleration and the recognition of the right of every man to judge for himself on all matters of opinion, that is, to allow himself to be guided by his reason, which demands sufficient evidence for belief. Already a remarkable change in the way of looking at things in all departments of thought has been brought about. To an extent before unknown each man now thinks for himself, and is no longer content to accept sluggishly the current beliefs of his time, but seeks to bring all things to the touchstone of experiment and experience.

Perhaps I am speaking a little prematurely, and painting the present under the illumination of the golden radiance of the dawn of a still freer future, for even to-day we are reminded in the Press, from time to time, that the spirit of persecution is not yet dead.

Another direction in which, during the last half-century, the public mind has been powerfully influenced by the discoveries and the methods of science, is in a change of attitude, in all matters of opinion, towards truth, by putting Truth for her own sake in the first place as its main quest.

I do not for a moment suggest that consciously the desire for truth does not take the first place in all honest hearts. All the other great departments of human interests, however, as politics, economics, theology and philosophy, are broken up into sharply divided schools of thought, of which the differences of opinion are accentuated by the jealousies and the intolerance of party feeling. In the great majority of cases, men find themselves, by the lot of birth and early education, among the adherents of one or other party, and nearly always come unconsciously to identify the issues of that particular party with truth itself. With the most honest intentions on the part of the speakers, the reasoning which is heard in Parliament, or from public platforms, is almost always one-sided, from the warping influence of party ties and issues.

In direct opposition to this narrowness of thought, which views all subjects through the distorting mirage of party prejudice, stands the absolute freedom

of mind of the man of science, who knows, or ought to know, nothing of party, and stands with open arms to welcome Truth in however strange or unexpected guise she may present herself. In his writings, the man of science has no lower aim than the diffusion of truth so far as it is known, and no desire to make converts to any opinion or party. As opposed to the finality of party opinions, he proclaims that truth is but very partially attained by man on any subject, for we can see Truth only imperfectly, as she appears altered by the perspective of our own standpoint. The scientific attitude of mind is no less than antipodal to that of the ordinary party man, wrangling for his own particular shibboleth.

Following upon greater freedom of private opinion, and the desire for truth rather than for party success, has grown up the greater fearlessness in suggestion, and in the acceptance of new views, which is undoubtedly characteristic of the present age, and stands in strong contrast to the conventional timidity of half a century ago. This fearlessness has been won chiefly through the widening of human thought by natural knowledge, by which the prejudices inherent in human nature, or which have come down by inheritance, have been greatly weakened, if not yet overcome. The fearless courage of change of opinion required by experimental science is safeguarded by the demand which she makes in all cases, for sufficient evidence from observation or experience.

To sum up, the influence of science during the last fifty years has been in the direction of bringing out and developing the powers and freedom of the individual, under the stimulation of great ideas. To become all that we can become as individuals is our most glorious birthright, and only as we realise it do we become, at the same time, of great price to the community. From individual minds are born all great discoveries and revolutions of thought. New ideas may be in the air and more or less present in many minds, but it is always an individual who at the last takes the creative step and enriches mankind with the living germ-thought of a new era of opinion.

All influences, therefore, and especially all laws and institutions which tend to lose the individual in the crowd, and bring down the exceptional to the level of the average, are contrary to the irresistible order of nature, and can lead only to disaster in the individual and in the State.

I should not omit to mention the marvellous secondary effects of scientific discoveries upon the mental progress of the civilised world which are being wrought by their practical applications to the cheapening of paper, and to improvements of the automatic printing-press, which, combined with the linking together of all parts of the earth by a network of telegraphic

communications, put it in the power of even the poor of the realm to read daily the news of the world, and for a few shillings to provide themselves with a library of classical works. Of scarcely less educational influence upon the public mind are the new methods of photography and mechanical reproduction, by which pictures of current events and the portraits of those who are making contemporary history, and also copies of the world's masterpieces of painting and of sculpture, are widely disseminated with the cheap newspapers and magazines among the mass of the people.

I have not spoken of the influence of science upon its own students, nor of the place it should take in general education. My purpose has been to point out the profound changes which science has wrought upon the habits of thinking of the general public, who themselves have no personal knowledge of science methods, changes which have revolutionised every activity of the human mind.

Golden will be the days when, through a reform of our higher education, every man going up to the Universities will have been from his earliest years under the stimulating power of a personal training in practical elementary science; all his natural powers being brought to a state of high efficiency, and his mind actively proving all things under the vivifying influence of freedom of opinion. Throughout life he will be on the best terms with Nature, living a longer and a fuller life under her protecting care, and through the further disclosures of herself, rising successively to higher levels of being and of knowledge.

As a corollary to what I have said, the place that science should take in general education, very briefly considered, will suitably occupy the few minutes which remain. I do not wish to speak of science as a specialised subject of advanced study, nor of technical education, which is obviously of supreme importance to all who look forward to finding their life-work in manufacturing and industrial pursuits, or of entering such professions as architecture and civil and electrical engineering.

The importance to every man of a practical acquaintance with elementary science is obvious. Would it be thought possible that any nation could act so absurdly as to teach its children other languages, and leave them in complete ignorance of the tongue of the land in which they would have to pass their lives? Would it not then be incredible, if it had not become a too familiar fact, that the public schools have, until recently, excluded all teaching of the science of nature from their scheme of studies, though man's relation to nature is more intimate than to his fellow countryman? We live, move, and have our being in nature; we cannot emigrate from it, for we are part of it. Yet our higher education leaves men, who in other directions are

well informed, much as deaf-mutes in the presence of Nature. They do not hear her most imperative warnings, and can only get on haltingly in their everyday intercourse with the natural forces to which their lives are subjected, by means of the arbitrary signs of empirical custom. The recent introduction of some amount of science-teaching in our higher schools is quite inadequate, alike in kind and in degree. It can be only through a reform of the scheme of their examinations by the Universities, that we can hope to see Science take the equal part with the humanities in general education to which she is entitled.

The place of science in general education may be considered under two different aspects: the intrinsic value of the teaching of science as a means of enlarging the powers of the mind; and secondly, its relative value and place as compared with the teaching of the classics.

The elements of the science of nature, when properly taught, have a claim to a very high place in early general education, since Nature is always close about us as a living intelligence and power, which responds to the questions put to her by experiment. The young mind finds itself no longer in the realms of the dead, deciphering from the inscriptions on their tombstones the history and opinions of past generations, invaluable as is such knowledge in its proper place, but in the open of light and life, where Nature holds her school, taking all things, great and small, as the object-lessons of her teaching.

Two faculties of the mind which it is of the highest importance, especially in early youth, to enlarge and develop by exercise, are wonder and imagination. Under the ordinary premature language-teaching of the Grammar Schools, even the wonder and imagination natural to young minds become so stunted in their growth, as to remain more or less dormant throughout life. On the other hand, natural science brings them into full activity and greatly stimulates their development. Nature's fairy tales, as read through the microscope, the telescope, and the spectroscope, or spelt out to us from the blue by waves of ether, are among the most powerful of the exciting causes of wonder in its noblest form; when free from terror it becomes the minister of delight and of mental stimulation.

And surely the master-creations of poetry, music, sculpture and painting, alike in mystery and grandeur cannot surpass the natural epics and scenes of the heavens above and of the earth beneath, in their power of firing the imagination, which indeed has taken its most daring and enduring flights under the earlier and simpler conditions of human life when men lived in closer contact with nature, and in greater quiet, free from the deadening rush of modern society. Of supreme value is the exercise of the imagination,

that lofty faculty of creating and weaving imagery in the mind, and of giving subjective reality to its own creations, which is the source of the initial impulses to human progress and development, to all inspiration in the Arts, and to discovery in science.

Further, elementary science, taught practically with the aid of experiment during a boy's early years, cannot fail to develop the faculty of observation. However keen in vision, the eyes see little without training in observation by the subtle exercise of the mind behind them. From the humblest weed to the stars in their courses, all nature is a great object-lesson for the acquirement of the power of rapid and accurate noting of minute and quickly-changing aspects. Such an early training in the simpler methods of scientific observation, confers upon a man for life the possession of an inexhaustible source of interest and delight, and no mean advantage in the keen competitions of the intellectual activities of the present day.

Training in the use of the eyes develops, at the same time, alertness of the intelligence, and suppleness of the mind in dealing with new problems, which, in after-life, will be of great value in facing unforeseen difficulties of all kinds, which are constantly arising.

Science, practically taught, does more, for, under the constant control of his inferential conclusions by the unbending facts of direct experiment, the pupil gradually acquires the habit of reasoning correctly from the observations he makes. In particular, he learns the most precious lesson of great caution in forming his opinions, for he finds how often reasoning, which appeared to him to be flawless, was not really so, for it led him to wrong conclusions. Further, from the constant study of nature, the student comes so to look at things as almost unconsciously to discriminate between those which are essential and those which are only accidental, and so, gradually, to acquire the faculty of classing the facts of experience, and of putting them in their proper places in a consistent system or theory. Are there any other studies, it may be asked, by which, in the same time, a young mind could develop an equally enlarged capacity for correct reasoning, and acquire so wide an outlook? Yet, notwithstanding the immense intrinsic value of its teaching, science is but one of the studies which are necessary for a wide and liberal education. Intellectual culture, or, in other words, the whole mind working at its best, requires, besides the training of all its powers harmoniously by the study of nature, an acquaintance with many other kinds of knowledge, especially of human history and the development of human thought, and of the human arts. Humanistic studies and experimental science are equally essential, and, indeed, complement each other. Either alone leaves the mind unequally developed, and its whole attitude one-sided,

and so produces a narrow type of mind, which is incapable of taking a wide view even of its own side of thought, and has but little sympathy with any subject outside it.

In the scheme of a liberal education, literature and languages, which include the habit of clear thinking in suitable words, should have a large place. It must, I think, be conceded that the languages of ancient Greece and Rome, which are highly developed for the conveyance of delicate shades of thought, still stand unsurpassed as means of training in thinking in association with correct expression, while, at the same time, they feed the mind with the great ideas, and the heroic deeds of the past.

In the methods of study of these languages, as actually carried out in the public schools, surely great reforms are possible. The complaint of the classicist, John Milton, who had been himself a schoolmaster, in his 'Tractate on Education,' written about twenty years before the foundation of the Royal Society, is urgently true to-day. He wrote: "We do amiss to spend seven or eight years merely in scraping together so much miserable Latin and Greek as might be learned otherwise easily and delightfully in one year." Later on Evelyn made a similar complaint. "At most schools," he wrote, "there is a casting away of six or seven years in the learning of words only, and that, too, very imperfectly."

Quite recently, the number of years usually given to Latin and Greek in the public schools has been shown by a striking experiment to be greatly excessive.

Last March, the Minister of Education gave an account in the Prussian Chamber of the so-called "reform schools," in which the study of the classics is begun for Latin at twelve, and for Greek not until the age of fourteen, with the encouraging result that, of 125 pupils who presented themselves for the leaving examination, only four failed to pass, and, of these four, three succeeded three months later. Experience showed that, as the result of beginning Latin and Greek at a later age, the interest of the pupils in their work was much keener, and their progress much more rapid.

Improved methods of teaching the classical languages which would permit of the beginning of the study of them at a later age, would leave ample time for an early training in experimental science, which must soon come to be recognised as an essential part of all education.

In future, no Grammar or Higher School should be considered as properly provided for unless furnished with the necessary apparatus for teaching experimentally the fundamental principles of mechanics, physics, and biology. The pupils should have the use of a small astronomical telescope, and of microscopes for biological work. Such apparatus and instruments can now be purchased at a very small cost.

Clearly, it is only by such a widening of the general education common to all who go up to the Universities, before specialisation is allowed, that the present "gap between scientific students careless of literary form, and classical students ignorant of scientific method" can be filled up, and the young men who will in the future take an active part in public affairs, as statesmen and leaders of thought, can be suitably prepared to introduce and encourage in the country that fuller knowledge and appreciation of science which are needed for the complete change of the national attitude on all science questions, which is absolutely necessary if we are to maintain our high position and fulfil our destiny, as a great nation.

I now proceed to the award of the Medals.

Copley Medal.

The Copley Medal is awarded to Professor Dmitri Ivanovitch Mendeleeff, For. Mem. R.S., for his contributions to chemical and physical science.

Professor Mendeleeff, born at Tobolsk, in Siberia, in 1834, stands high among the great philosophical chemists of the last century. As early as 1856 he published his own conclusion that paramagnetic elements have, in general, smaller molecular volumes than diamagnetic elements, and confirmed Avogadro's view that electro-positive elements have larger molecular volumes than electro-negative ones: both of them results specially interesting in connection with modern views of molecular structure. At that time he had already assimilated and utilised the views of Laurent, Gerhardt, and Williamson on molecular constitution, which made such slow progress in general. Since then, in the words of Dr. Thorpe ('Nature,' June 27, 1889): "There is, in fact, no section of chemical science which he has not enriched by his contributions"—mineralogy, chemical geology, organic chemistry, the nature and industrial importance of petroleum, but, above all, physical chemistry and chemical philosophy.

Quoting again from Dr. Thorpe: "His 'Principles of Chemistry,' published in 1889, and repeatedly reprinted, is a veritable treasure-house of ideas, from which investigators have constantly borrowed suggestions for new lines of research. This book is one of the classics of chemistry; its place in the history of science is as well assured as the ever-memorable work of Dalton." In the course of its preparation he developed the great generalisation known as the Periodic Law of the Elements, with which his name will ever remain most closely associated, especially as a weapon for predicting new elements, and for which he has received the Davy Medal of this Society, as also have Newlands and Lothar Meyer for their independent advances in the same direction.

This law has changed the face of chemistry, by imparting to the study of its numerous independent elements that close inter-connection which is a characteristic of advanced physical theories.

Royal Medals.

A Royal Medal is awarded to Professor John Henry Poynting, F.R.S., on account of his researches in physical science, especially in connection with the law of gravitation and the theories of electro-dynamics and radiation.

Professor Poynting is distinguished both in theoretical and experimental physics. His memoir, 'Phil. Trans.,' 1884, "On the Transfer of Energy in the Electro-magnetic Field," contains the fundamental proposition which is now universally known as Poynting's Theorem. It was followed in 'Phil. Trans.,' 1885, by a paper "On the Connection between Electric Current and the Electric and Magnetic Inductions in the Surrounding Field," which works out the current circuit on the supposition of motion of what are now called Faraday Tubes. These papers served greatly to elucidate Maxwell's theory, and give a representation of the physical nature of the electric field which is now widely utilised. His long-continued experimental and theoretical researches on the Constant of Gravitation and on the Mean Density of the Earth are reported in a paper in the 'Phil. Trans.,' 1892, and in the Adams' Prize Essay for 1893. Closely related to this subject is an experiment in search of a directive action of one quartz crystal on another, 'Phil. Trans.,' 1899, which, though leading to a negative result, is a model of the application of refined methods to a physical research of great delicacy. His recent paper, 'Phil. Trans.,' 1903, "On Radiation in the Solar System, its Effect on Temperature, and its Pressure on Small Bodies," is of great interest and significance in cosmical physics. He is the author of various theoretical papers on physico-chemical subjects, such as change of state and osmotic pressure, which are conspicuous for originality of conception and clearness of exposition.

The other Royal Medal is awarded to Professor Charles Scott Sherrington, F.R.S., for his work on the Central Nervous System, especially in relation to Reflex Action.

Professor Sherrington has published a series of important papers upon the structure and function of the brain and spinal cord. In the earlier of these he chiefly investigated the course of the several groups of nerve fibres by means of the degeneration method. Passing from the study of structure to that of function, he discovered that removal of the fore brain causes a wide-spread rigidity of certain muscles, which he called decerebrate rigidity. In

the state of decerebrate rigidity, the ordinarily observed reflexes of the body become profoundly altered, and a study of the normal and abnormal reflexes led him to the observation that contraction of one muscle is commonly associated with inhibition of its antagonist. Upon this he formulated the law of the Reciprocal Action of Antagonistic Muscles, which is now accepted as of fundamental importance in the co-ordination of muscular movement. A further study of reflex actions led him to lay down certain general principles with regard to them. One principle deserves especial mention, namely, that hurtful stimuli applied to the skin produce a different form of reflex from that given by stimuli which are not hurtful. This has served as a basis for further investigation on the character of the nerve impulses conveyed by different nerve-endings, on the course taken by the impulses, and on their central connections.

In recent years a considerable amount of work has been done in mapping out the areas of the skin supplied by each of the cranial and spinal nerves. This work, essential both to physiology and to clinical medicine, received its chief impetus and most weighty contribution from the careful and detailed observations of Professor Sherrington.

The researches of Professor Sherrington and Dr. Grünbaum, on the localisation of the excitable areas in the cortex of the cerebral hemispheres in the higher apes, having resulted in placing the "motor area" in this animal entirely in front of the central sulcus. The result is now generally accepted as true also for the brain of man—a point of great importance in the surgery of the brain.

Professor Sherrington's researches have dealt with a number of subjects cognate with that of the central nervous system. He has shed light on questions connected with the afferent nerves of skeletal muscle, the efferent nerves of the arrectores pilorum and of the cranial blood-vessels, the innervation of various viscera, the trophic centre of the fibres of the roots of the spinal nerves, the knee jerk, and with the physiology of vision.

Davy Medal.

The Davy Medal is awarded to Professor Albert Ladenburg, on account of his researches in organic chemistry, especially in connection with the synthesis of natural alkaloids.

Thirty years ago, when the validity of Kekulé's famous formula for benzene was the subject of much discussion, Ladenburg was the first to prove, by laborious research, the important proposition that the six hydrogen atoms in the hydrocarbon are similarly situated and discharge the same functions, and hence that three and only three *di*-substitution derivatives can exist.

He has also devoted many years to the study of the natural alkaloids. This pioneer work, attended by many experimental difficulties, was rewarded by success in the synthesis, for the first time, in 1886, of an optically active compound identical with the alkaloid coniine existing in the hemlock plant. Since that time he has largely added to our knowledge of the chemistry of hyoscyamine, atropine, and other alkaloids of the mydriatic class.

Hughes Medal.

The Hughes Medal is awarded to Professor Augusto Righi, for his experimental researches in electrical science, including electric vibrations.

Professor Righi has been for many years a prominent and active worker in the sciences of light, electricity, and magnetism.

Among the subjects which have engaged his attention are the Hall effect, and the change of electric conductivity of bismuth in a magnetic field. At an early period he carried out an elaborate investigation on the reflection of light at the surface of a magnetised body, repeating and extending Kerr's observations with more powerful apparatus; in particular, he showed how the amount of the rotation of the plane of polarisation depends upon the wave-length of the light.

A valuable series of papers related to phenomena produced by the ultraviolet rays, including the first discovery of the discharge of negative electricity from a freshly polished not previously electrified zinc surface under their influence. He has also investigated the potential in the neighbourhood of the cathode in a Crookes' tube, and made many experiments on the spark discharge in gases, and the action of the Röntgen rays.

His work on electric radiation has been collected in a book, 'L'Ottica delle oscillazioni elettriche,' Bologna, 1897. He rendered fundamental service to exact experiment on this subject by simplifying the practical conditions of the problem; and he applied his improved apparatus to numerous investigations on the behaviour of electro-magnetic waves, of short and therefore manageable wave-length, under very varied conditions, on their absorption, polarisation, reflection and refraction, and on the behaviour of dielectrics in the field of radiation. This work entitles him to a high place among those who developed the lines of experimental investigation opened up by the great discoveries of Hertz.

More recently he has contributed substantially to the study of the phenomena of radio-activity and the related ionisations.

As I address you now for the last time, I wish to say how fully I have

appreciated the honour—the crowning honour—which can fall to the lot of but few Fellows, which I have received at your hands. Most deeply have I felt the great responsibility associated with this honour, and during a not uneventful period, it has been my most earnest endeavour to uphold, as far as it lay in my power, the high traditions of our great and ancient Society.

In bidding you farewell, I desire to express to the entire body of the Fellows my gratitude for their invariable consideration and courtesy, and in particular to the Officers who have served with me, my warm thanks for their efficient support and assistance, and for the thoughtful and prevenient attention by which they have sought to lighten the duties of my office.

I rejoice that in the hands of my probable successor, a man of world-wide eminence in science, the interests and the reputation of the Society are eminently safe.

Farewell! *Floreat Regalis Societas Londini!*

A Gas Calorimeter.

By C. V. Boys, F.R.S.

(Received December 7,—Read December 7, 1905.)

In the report of the gas section of the Electrical and Gas Exhibition held at the Crystal Palace in 1884, there is an illustrated account of a gas calorimeter devised by Mr. F. W. Hartley for the purpose of testing the calorific value of the gas used in the cooking and heating apparatus shown in the exhibition. This report, which is rather inaccessible, may be seen in the library of the Patent Office in Southampton Buildings, Chancery Lane, and its catalogue number is 15,262.

In this instrument the gas to be examined is passed through a delicate meter and governor, after which it is burned in a Bunsen burner in the instrument. The heat is taken from the products of combustion and the unchanged air by means of a stream of water, the rate of flow and rise of temperature of which are determined. From these observations the calorific value of the gas can be deduced. Mr. Hartley considered the very small corrections due to the effluent gas being slightly different in temperature from the surrounding air, also corrections due to adventitious loss or gain of heat, and to the change in the specific heat of water with temperature. The paper unfortunately occupies only five pages, and the construction of the details of the instrument can only be inferred from the figure.

The Hartley type of apparatus has in the last 14 years become well known owing to the sale of the excellent instrument designed by Junkers. I have had occasion to use this instrument a good deal, and while I in no way criticise it on the ground of want of accuracy, I have found it unwieldy and inconvenient in use. If the water and gas supply and the water escape as provided suit the surroundings, well and good, if not, pipes must be taken round to adapt the surroundings to the instrument. The thermometers for the inlet and outlet water are at very different levels and the instrument is inordinately high, so that if placed on a table or by the side of an ordinary sink the observer has to keep climbing up and down in order to read the two thermometers and make notes. The thermometers and the reading-glasses provided are admirable; in this respect improvement appears impossible, but the readings easily made to $1/100^\circ$ C. are vitiated by the constant spasmodic jumps of the indication of the outlet thermometer in consequence of warmer and cooler streaks in the issuing water. These irregularities may amount to $1/10^\circ$ more or less, and

though they are not enough to introduce error of consequence, they give the opportunity to different observers to read on the whole high or low, and they may encourage the idea that accuracy in reading the thermometer is not of much consequence. The water content of the instrument is 1700 c.c., and so, with a usual rate of flow, from two to three minutes are required to change the water. This large content and the consequent time of change must be taken into account if the temperature of the inlet water is undergoing a small but steady variation at a rate sufficient to be observed in this time. Simultaneous measures of inlet and outlet temperature would not give the true rise of temperature; to approximate to this the inlet readings should be taken two or three minutes before the corresponding outlet readings, the interval depending on the rate of flow. A more serious objection to a large water capacity is the time that the observer must wait before the final temperature is reached, the last creeping up being very slow.

I have also experimented with two other makes of gas calorimeter, but I have found that they are little more than copies of the Junkers instrument. The spasmodic variations of the outlet readings are quite as great; the water content in one of them is nearly 6 litres, and nothing has been done in either to reduce the inconvenient height or to increase the facility with which they may be adapted to their surroundings.

Professor Threlfall has designed a calorimeter with the view specially to keeping a continuous record of the variation of the calorific power of gas. I have not seen this instrument or any complete account of it, but Professor Threlfall has informed me that he has kept the water content as low as he could.

I have had to examine existing calorimeters in consequence of a change in the law with regard to the testing of gas in London. From January 1 next the calorific value is to be taken, and it is the duty of the gas referees to prescribe the mode of testing. From a purely official point of view the three calorimeters examined have the disadvantage that they are soldered up, so that it is impossible to tell what is inside them or to see if they are made according to instructions.

The calorimeter shown in section in fig. 1 will, I believe, be found to be free from the disadvantages enumerated, and it has the peculiarity that it can be taken entirely to pieces in a few minutes so that all parts may be examined.

A circular base A carries the gas pipe and tap terminating in a pair of No. 3 union-jet burners B. The top surface is protected from radiation by a disc of bright metal which is held down by the screws which fix the three centering blocks C. In the base a governor may be inserted or an

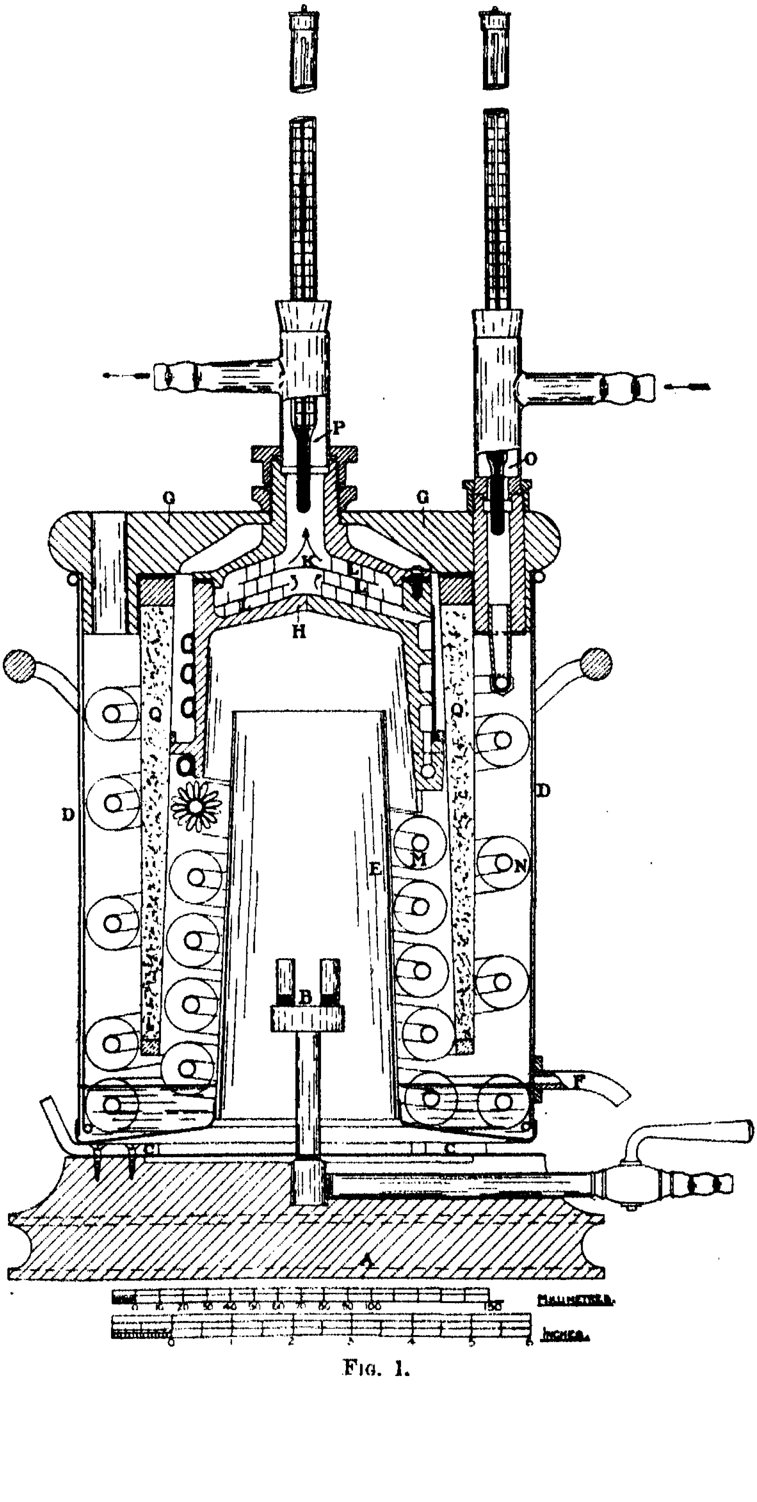

P
O
G
G
K
L
L
H
Q
Q
D
D
M
N
E
B
F
C
C
A
MILLIMETRES.
0 10 20 30 40 50 60 70 80 90 100 150
INCHES.
0 1 2 3 4 5 6

FIG. 1.

ordinary balance governor may be used instead to counteract the variation in pressure resulting from the working of the meter.

On the three centering blocks C rests a vessel D of sheet brass with a central chimney of thick sheet copper E. On one side, 1 inch from the bottom, a side tube F is fastened so that condensation water may drip from this into a measure placed to receive it. Of course, the vessel D may be turned round so that the drip tube lies in any direction with respect to the gas inlet.

Attached to the lid G are the essential parts of the calorimeter. Beginning at the centre, where the outflow is situated, there is a brass box which acts as a temperature equalising chamber for the outlet water. Two dished plates of thin brass K K are held in place by three scrolls of thin brass L L L. These are simply strips bent round like unwound clock springs, and no attempt should be made to prevent all leakage from one spire to the next, as a little will be advantageous in encouraging temperature equalisation. For the same reason a little leakage from each spire to the one above may be allowed. The lower or pendant portion of this box is kept cool by circulating water, the channel for which may be made in the solid metal as shown on the right side or by sweating on a tube as shown on the left. Connected to the water channel at the lowest point by a union are six turns of copper pipe such as is used in a motor-car radiator. I have employed a helix of copper wire wound round the tube and all sweated together, the well-known invention of Mr. T. Clarkson of Chelmsford, who kindly sent me the wire for the purpose. A piece of such tube is shown in fig. 2. I have no doubt, however, that squares of sheet copper, as shown in fig. 3, threaded on

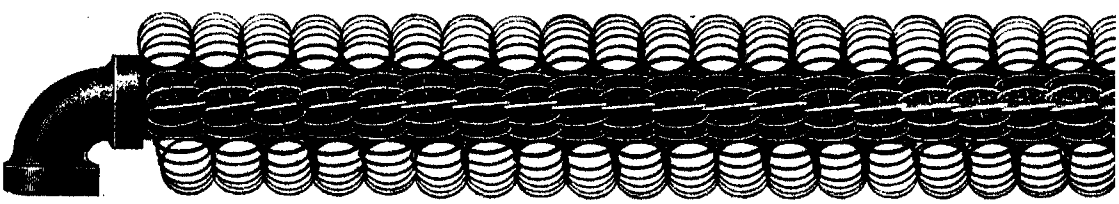


Fig. 2.

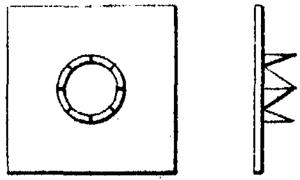


Fig. 3.

to the pipe and all sweated together or a series of radially disposed copper strips cut so as to allow the pipe to be wound upon them, and then be bent over outside so as to make good sweating contact with it would answer as well. A second helix of similar pipe surrounding the first is fastened to it at the lower end by a union. This terminates at the other end in a block to which the inlet water box and thermometer holder are secured by a union as shown at O. A similar outlet water box P and thermometer holder are similarly secured above the equalising chamber H. The lowest turns of the two coils M and N are immersed in the water, which in the first instance is put into the vessel B.

A further alternative for the inner but not the outer coil, which would, no doubt, answer well, could be made by winding a sheet of copper wire gauze, six wires to the inch, into a cylindrical form, winding the pipe upon the gauze and a second sheet of gauze upon the pipe and then sweating all together.

Between the outer and the inner coils M N is placed a brattice Q made of thin sheet brass, but containing cork dust to act as a heat insulator. The upper annular space in the brattice is closed by a wooden ring and that end is immersed in melted rosin and beeswax cement to protect it from any moisture which might condense upon it. The brattice is carried by an internal flange which rests upon the lower edge of the casting H. A cylindrical wall of thin sheet brass a very little smaller than the vessel D is secured to the lid, so that when the instrument is lifted out of the vessel and placed upon the table the coils are protected from injury. The narrow air space between this and the vessel D also serves to prevent interchange of heat between the calorimeter and the air of the room.

It will be noticed that the two thermometers for reading the water temperatures, and a third which may be added for reading the temperature of the outlet air, are all near together and at the same level, and that the lid may be turned round into any position relatively to the gas inlet and condensed water drip that may be convenient for observation, and also that the inlet and outlet water boxes may themselves be turned so that their branch tubes point in any direction. The instrument is convenient also in its small height, the thermometers being comfortably read when the instrument is standing on an ordinary table.

For regular testing purposes there is no need to use different rates of flow of water at different times. I therefore fasten an overflow water funnel (fig. 4) on the wall at a convenient height over the sink and connect it by indiarubber tubes with a supply tap from the main and with the inlet O of the instrument. A uniform rate of flow is most easily attained by the use

of a diaphragm in the supply pipe, which has been reamed out to allow the desired flow to pass through the instrument with the given head. There will be no occasion afterwards to adjust this.

The flow of air to the burner is determined by the degree to which the passage is constricted at the inlet and outlet. I have found that if the three centering blocks are made of material 3/16 inch thick, and the gas escapes by five holes 5/8 inch in diameter made in the outer portion of the lid, the flames burn well, do not smoke, and an unnecessary excess of air is prevented from passing through the instrument. A sixth hole may be provided for

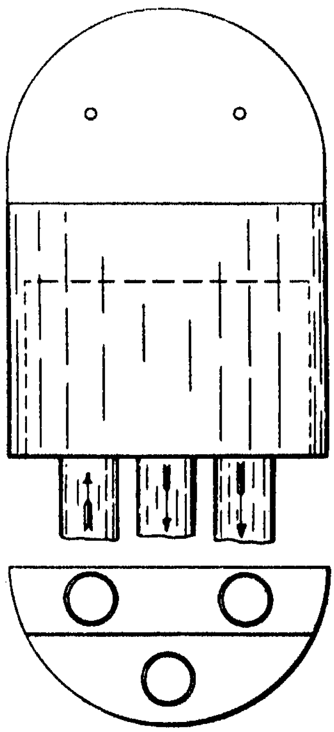


FIG. 4.

a thermometer, so placed as to measure the temperature of the outlet gases.

Mr. Butterfield has examined the escaping gases and finds that when the gas is burning at the rate of 5 cubic feet per hour they contain 7 per cent. of oxygen and 8 per cent. of carbonic acid, from which he infers that the proportion of air to gas is about 8½ to 1. It follows from this that the correction for rise or fall of temperature of the air passing through the instrument is as near as possible one-third of a calorie to the cubic foot of gas for each degree Centigrade of change. An examination of the effluent

gas when only four holes were left open showed that the oxygen was reduced to 5·6 per cent., but that even so no trace of combustible gas could be detected.

A feature peculiar to this calorimeter is the absence of water cooling from all but the lower end of the chimney; being made of copper 1/16 inch thick, and with its base in contact with water it is prevented from becoming hot enough to burn, while at the same time it is so hot that condensation does not occur upon its inner surface, and so the collection of the condensation water is simplified. Another advantage of the hot chimney is its extra aëro-motive power, so that the instrument can safely be made of far less than the usual height. The heat passing by condensation down the chimney into the water pool at the bottom is carried away by the water in the two immersed turns of pipe. Heat radiated downwards is in part reflected back by the bright metal shield over the base, but in so far as the reflection is imperfect and the thin metal is warmed it gives up most of its heat to the incoming air which sweeps over it and so carries it back into the instrument.

It may be well to state that the object of measuring the condensation water is to find how much of the total heat is due to condensation of steam and cooling of the water down to the temperature of the drip. If this is subtracted the result will give the heating value of the gas for operations in which the steam is not condensed, and this is generally known as the net as distinct from the gross calorific value.

In order to prevent corrosion of the metal surfaces by the continued soaking action of very dilute sulphuric acid and dissolved oxygen, the whole of the coil system can be lifted up out of the vessel D when the measurements have been made and placed in a jar containing a very dilute solution of carbonate of soda. This source of destruction is likely to become more damaging in the near future than it has been in the past in consequence of the great increase in the amount of sulphur which, without lime purification, will be left in the gas. Any deposit of lime that may be formed in the pipe system and equalising box can be removed by passing very dilute hydrochloric acid through and washing out with water. By these means it is hoped that the calorimeter will be available for daily use and be practically indestructible.

When I first considered how best the irregular outlet temperature of existing calorimeters could be made more uniform, I concluded that an inversion of the usual arrangement was essential, that, in fact, the gases should have plenty of space to pass gently through the instruments and that the water should be taken through every channel strictly in series, all alternative parallel flow being prejudicial. The small pipe suitable for such

construction, provided it is fortified with heat collecting ribs or wires, can still carry abundant water and absorb the whole of the heat and it has very small water capacity. At any rate such a construction, with the additional equalising box, shows at times irregular variations of only 0·01° or 0·02° even with the total rise of 24° C. and without any device for in part shielding the thermometer bulb from direct contact with the stream of water. The water content of the coil and equalising box is 300 c.c., and of the space in the vessel D up to the overflow 400 c.c.

It will be seen from fig. 5, in which are plotted observations of the

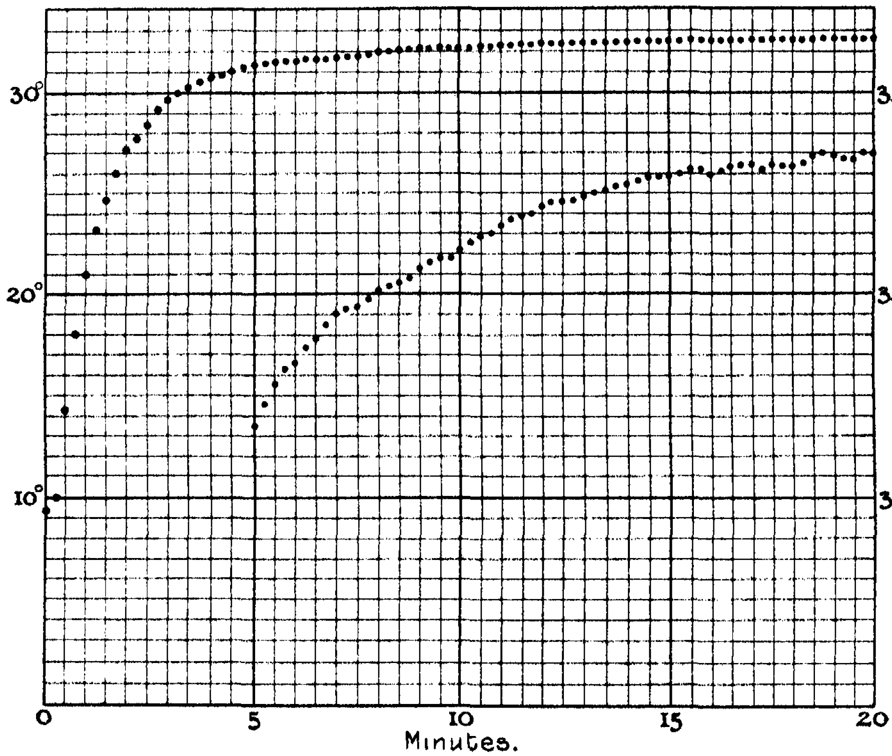


Fig. 5.

outlet temperature made every quarter minute from the time of lighting the gas, that five minutes after lighting up 21° out of the 22° rise have been attained, that in 10 minutes a further rise of ½° has occurred, and in a quarter of an hour there is only 1/10° remaining. A reading taken 55 minutes after starting showed the outlet water to be at 32·85°.

The rise of 0·15° in the last 35 minutes is no more than that which corresponds to the usual rise of calorific value of the gas in Victoria Street

taken at intervals during the day. The small waves in the portion of the curve drawn to 10 times the scale are the result of friction in the meter, in consequence of which the governor was in constant motion.

I must express my indebtedness to Messrs. Griffin and Sons, of Sardinia Street, who made a trial instrument for me when I was away in South Africa, and therefore out of reach. They have carried out my instructions accurately, and to my entire satisfaction.

On the Spectrum of the Spontaneous Luminous Radiation of Radium. Part IV.—*Extension of the Glow.*

By Sir WILLIAM HUGGINS, K.C.B., O.M., D.C.L., F.R.S.; and Lady HUGGINS, Hon. Mem. R.A.S.

(Received December 12,—Read December 14, 1905.)

In our second paper* we suggest "whether the β-rays, which are analogous to the cathode corpuscles, may not be mainly operative in exciting the radium glow. On this surmise it would be reasonable to expect some little extension of the glow outside the limit of the solid radium itself. We are unable to detect any halo of luminosity outside the limit of the solid radium bromide; the glow appears to end with sudden abruptness at the boundary surface of the radium." We omitted to state that this conclusion was arrived at by eye observations. The radium was observed in the dark with a lens, and with a low-power microscope.

The earlier photographs of the spectrum of the glow were taken, for the purpose of comparison spectra, with the height of the slit reduced by shutters so as to be within the width of the exposed radium bromide, and, therefore, these photographs would not show whether the bright bands of nitrogen extend into the air beyond the radium. Subsequently photographs were taken with the whole height of the slit, and on these we find that all the bands of nitrogen do extend to some little distance outside the radium salt. Our attention at the time being directed to other phenomena of the glow, we did not examine the photographs to see if the nitrogen bands extended beyond the radium.

In a paper, dated August 22, 1905, F. Himstedt and G. Meyer† state that

* 'Roy. Soc. Proc.,' vol. 72, p. 410 (1903).

† F. Himstedt and G. Meyer, 'Ber. d. Nat. Gesells. Freiburg,' vol. 16, pp. 13—17.

in their photographs of the spectrum of $RaBr_2$, the four nitrogen bands, 3577, 3371, about 3300, and 3159, extend beyond the radium salt, while the other less refrangible bands are not traceable outside the radium. In our photographs all the nitrogen bands project beyond the radium salt; the relative distance to which the extension can be detected in the case of each band being, as might be expected, in proportion to the strength of the impression of that band upon the photographic plate.

B. Walter and R. Pohl, in a paper, dated September, 1905,* give an account of experiments made with the help of screens, which show that for a distance of up to about 2 cm., the air surrounding radium bromide has an action on a photographic plate.

On re-examining an early photograph, taken in 1903 for another purpose, which is described in our second paper,† in which the $RaBr_2$ was enclosed in a very narrow tube of thin glass, we find that the bands of nitrogen, which are strong within the tube, show no trace of extension on the plate beyond the tube. The exposure of this plate was seven days.

This experiment, which we have repeated recently with an exposure of 14 days, shows that the luminosity of nitrogen in the near neighbourhood of radium bromide is not due to the cathode-like β-radiation, for this passes freely through glass.

Two explanations may be suggested: first, that the active cause is the α-rays;‡ or secondly, that the nitrogen molecules which encounter those molecules of the radium which are undergoing active changes are broken up into ions, which are projected outwards, and give rise to the glow of luminous nitrogen.§

* B. Walter and R. Pohl, 'Ann. d. Phys.,' vol. 18, p. 406.

† 'Roy. Soc. Proc.,' vol. 72, p. 412.

‡ B. Walter, July, 1905, showed by means of absorption screens that the radiation from radio-tellurium can produce the ultra-violet light of nitrogen ('Ann. d. Phys.,' vol. 17, p. 367).

§ The experiments described in our last paper showed that probably the β-rays are not the operative cause of the nitrogen glow ('Roy. Soc. Proc.,' vol. 76, p. 488).

First Photographs of the Canals of Mars

By Professor PERCIVAL LOWELL, Flagstaff Observatory, Arizona.

(Communicated by Sir Norman Lockyer, K.C.B., F.R.S. Received September 27,
—Read November 16, 1905.)

[PLATE 1.]

To make the canals of Mars write their own record on a photographic plate, so that astronomers might have at first hand objective proof of their reality, has long been one of the objects of this observatory. The endeavour has at last succeeded. Unnecessary as such corroboration was to the observers themselves, it is different with the world at large; for the work of the camera at once puts the canals in a position where scientists in general, as well as astronomers in particular, are able to judge the phenomena.

The difficulties in the way, however, at first proved insuperable. The main markings of the planet were secured by the camera here four years ago, but to get the canals to show was a matter of an altogether different order of difficulty from that of celestial photography in general. This will be appreciated on recalling Richey's excellent photographs of the moon, within the wall of one of whose smaller craters the whole disc of the planet might be enclosed. When it is further considered that the delicate detail on this disc bears to it the same relative ratio that the craters themselves do to the whole moon, the almost impossible task of reproducing the canals will be understood.

After unsuccessful attempts at the last two oppositions, results were finally secured by Mr. Lampland's great skill and long-continued study of the subject. A preliminary notice of the result was given in Bulletin No. 21, in which the prints left something to be desired, but the present paper presents them in a later and more perfected form.

The difficulties encountered were two-fold: the securing instrumental means of a high enough order of delicacy, and the taking of the photographs in such a manner as to minimise the destructive effects of the air-waves. Both obstacles were overcome by the combination of the following conditions.

First, the glass used was the 24-inch refractor of this observatory, which recent tests have shown to be, from the favourableness of its position, the most space-penetrating glass at present in use. In a chart of the region following δ Ophiuchi it shows 172 stars, the Lick chart of the same region showing 161, and the newly added stars being all of the 16th or 17th magnitude. The Washington Naval Observatory glass showed in this region

63 stars. This result is partly due to the glass, which is Clark's latest work, and one of Mantois' most homogeneous meltings, being almost absolutely free of bubbles and striæ, but chiefly to the air, which is at once unusually transparent and steady. The site was chosen on this account, though it was not thought that the difference would amount to a whole magnitude better than the Lick, which, in view of the greater size of the Lick glass, these tests show to be the case. Mr. Slipher's tests of spectrographic exposure times as compared with those recorded for the Yerkes glass point to the same result.

Second, a knowledge of the atmospheric conditions needed to succeed has been one of the main studies of this observatory for the last ten years, and it is to the outcome of these researches that the result is in large part due. It has been found here that the air-waves were detrimental in two ways, depending upon their size relatively to the glass. They are made up of trains of waves of condensation and rarefaction, and if the distance from crest to hollow be equal to the diameter of the object-glass, the train will produce a bodily oscillation of the whole image in the field of view; if, however, the wave-length be shorter than this, partative motion occurs, while the bodily motion is reduced, the result being that we have an apparently steady image, but a blurring, and finally a complete obliteration, of the delicate detail.

It is to this fact that is due much of the misconception on the subject; the image often appears to be perfectly shown and yet discloses either no fine detail or else shows such only in a blurred and indefinite condition. This is the reason the canals are often reported to be streaks, whereas under better atmospheric conditions, *i.e.*, when the relatively small waves are absent, they appear as they really are, very narrow dark lines.

The other aspect is produced by the blurring tremor of the air-waves, the real image of the canal being thus spread out and consequently diffused. Both aspects have been seen and studied here, so that we are certain of their relation to one another. The larger the glass the more likely is this state of confused illusion to occur, a knowledge of which suggested to us the diaphragming-down of the 24-inch objective, with a result which was really surprising. It was found very rare that the definition was not improved by the artifice; in the exceptional moments the full aperture absolutely corroborates the small ones.

This same device was next applied to photography, and the camera entirely corroborated the evidence of the eye. This is an important fact, inasmuch as it shows that ninety-nine times out of one hundred, which was our experience, a smaller aperture shows planetary detail better than a large one; in the one hundredth case the large glass is able to assert its superiority and then

entirely corroborates the clear-cutness of all the smaller apertures had shown. This is the second point that made these photographs possible. The objective was diaphragmed-down to suit the particular wave-currents travelling at the moment.

Third, a colour screen was made by Mr. Wallace, the well-known maker of colour screens, to suit exactly the colour curve of the objective, so that it should let pass those rays only where the curve was at its flattest and therefore the focus the same for all. Fourth, Cramer's isochromatic plates were used, which, from their colour curve, carry the definiteness on to the photographic plate. Fifth, the camera was made movable, so that many images might be taken consecutively on one and the same plate; for detail of the delicateness of the canals shows or hides according to the air-waves of the moment, coming out in the lucid intervals perfectly distinctly, but being blurred in confusion after the manner of the twinkling of a star when the light waves are unfortunately refracted. Sixth, a particularly steady driving clock.

About seven hundred images of the planet were secured in this way during the time that it was sufficiently favourably placed for such work at the opposition which has just passed. The time available was only while the planet subtended a disc of about 15 seconds of arc, a smaller disc rendering the details of the image too nearly of a size with the silver grains of the emulsion. These plates, when sufficiently good, all show the canals, but as it is not practicable to reproduce them all, owing to the great difficulty and expense of the matter, a few have been selected at intervals round the planet.

The photographs show that, within the limits imposed by the silver grain of the plates, the canals are lines, narrow and direct, following either arcs of great circles or curving (like the Djihoun) in a systematic manner. In other words the photographs prove up to the extent of their ability what observations by the eye here have asserted. The eye is able to go much further than the camera, and the better these strange markings are seen under the best conditions yet procurable (at Flagstaff) the stranger they show. That the camera confirms, as far as it can go, the eye observations at Flagstaff, should lead any unprejudiced mind to consider very seriously the probability of their being correct beyond.

At first glance the several diverse parts of the planet may be instantly recognised, and on more careful scrutiny a great many canals will be seen recorded. In looking at them it should be remembered that, in order to minimise the time of exposure sufficiently, a large-grain plate had to be used, and more than a very slight magnification of these already enlarged prints will suffice to make that grain disturbingly prominent. A careful examina-

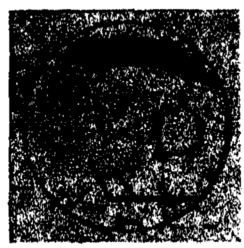
FIG. 1.

λ = 20° June 6, 9 h. 8—12 m.

λ = 17° June 9, 10 h. 49 m.—11 h. 7 m.

FIG. 2.

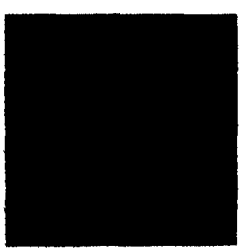
λ = 194° May 20, 10 h. 42½—45 m.



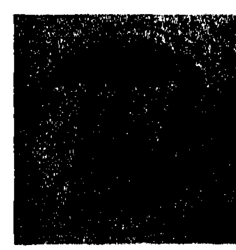
λ = 190° May 20, 10 h. 25—40 m.

λ = 171° May 22, 10 h. 20—30 m.

FIG. 3.

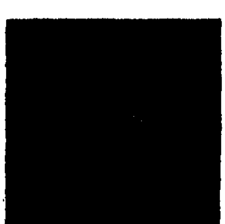


λ = 265° June 19, 9 h. 25 m.



λ = 249° June 19, 8 h. 17—26 m.

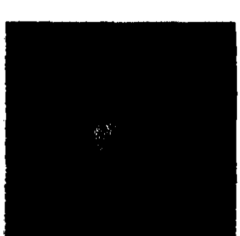
FIG. 4.



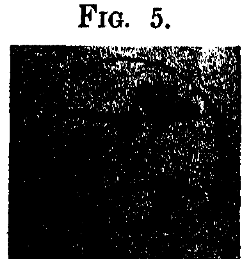
λ = 300° May 11, 12 h. 31—38 m.

λ = 299° May 6, 10 h. 42—52 m.

FIG. 5.



λ = 348° June 9, 8 h. 48—50 m.



λ = 356° June 9, 9 h. 20—29 m.

λ = 340° June 11, 9h. 30—39 m.

tion, however, will show that any discontinuity observed in these markings is due to the grain and not to the marking itself. Indeed, though I do not care to assert it now, there is evidence from these plates that both a double canal and a double oasis have been photographed.

The prints here presented were obtained after those on which the original announcement was made, and the process (by Mr. Lampland's skill) has in them been brought to greater perfection. Side by side with the prints are given reproductions of my own drawings, made absolutely independently of the photographs and selected for about the regions and the times at which the photographs were made. Sometimes the canals in the photographs actually appear better than they do in the drawings, though, of course, the eye is a very much more powerful instrument than the camera, because the camera must register the bad moments with the good, and details perfectly distinct to the eye must not be expected in the prints in consequence. To produce their true effect the prints should be looked at either without further magnification or with only a very slight one, for the grain of the plate will soon destroy the true character of the detail.

DESCRIPTION OF PLATE.

FIG. 1.—Margaritifer Sinus region, 1905.
FIG. 2.—Sinus Titanum region, 1905.
FIG. 3.—Aquæ Calidæ region, 1905.
FIG. 4.—Syrtis Major region, 1905.
FIG. 5.—Aeria region, 1905.

λ = Martian meridian central at the time.

The Periodogram and its Optical Analogy.

By ARTHUR SCHUSTER, F.R.S.

(Received November 23,—Read December 7, 1905.)

I have recently applied the periodogram method to the investigation of several fluctuating quantities, and the experience thus gained has led me to modify slightly the original definition.* Having always laid stress on the fact that the periodogram supplies by calculation the transformation which the spectroscope instrumentally impresses on a luminous disturbance, I may now enter a little more closely into this optical analogy, and thus lead up to what I hope will be the final definition.

Consider a parallel beam of light falling on a grating, the reflected light being collected at the focus of an observing telescope in the usual way. For simplicity of calculation I assume that the grating considered is of a particular type, which, in a former paper, I have called a simple grating. Such a grating only gives two spectra of the first order.

If $\phi\,(\mathrm{V}t+x)$ be the velocity at any point of the incident beam, the displacement at the focus of the observing telescope is†

$$\frac{hl\cos\beta}{2\pi f\mathrm{NV}\lambda}\,\mathrm{R},$$

where

$$\mathrm{R}=\int_{-\frac{1}{2}\mathrm{N}\lambda}^{+\frac{1}{2}\mathrm{N}\lambda}\cos nx\,\phi\,(\mathrm{V}t+x)\,dx. \qquad (1)$$

In these equations f denotes the focal length of the telescope, h is the length of the lines ruled on the grating, l the width of ruled space measured at right angles to the lines, N gives the number of lines, and β the angle between the direction of the optic axis of the observing telescope and the normal to the grating. For the sake of shortness n is written for $2\pi/\lambda$. The quantity denoted by λ is the wave-length of homogeneous light which would have its first principal maximum at the focus of the telescope. It may be said to be the wave-length towards which the telescope points, and its strict definition is given by the relation

$$l\,(\sin\alpha-\sin\beta)=\mathrm{N}\lambda,$$

where α is the angle of incidence.

In order not to complicate needlessly the calculations, I shall assume that the resolving power is sufficient to ensure that at any point of the spectrum

* 'Cambridge Phil. Soc. Trans.,' vol. 18, p. 107.

† 'Phil. Mag.,' vol. 37, p. 545 (1894).

the vibrations are nearly homogeneous; this involves that the average squares of the velocities are sensibly equal to the average squares of the displacements multiplied by $4\pi^2 V^2/\lambda^2$. The average square of the velocity at the focus of the telescope is in that case—

$$\frac{h^2 l^2 \cos^2\beta}{f^2 N^2 \lambda^4} R^2.$$

where for R^2 we must put its average value. This expression represents the measure of the intensity at the point considered. Its line and surface integrals may be called the total linear intensity and the total intensity respectively.

In observing a spectrum, we associate with a particular wave-length all the light which lies in a straight line parallel to the rulings of the grating. The distribution of light along a vertical line for nearly homogeneous light takes place according to the law $\alpha^{-2}\sin^2\alpha$, where $\alpha = \pi hy/f\lambda$, y being the vertical distance. Multiplying by dy, and integrating from minus to plus infinity, the total intensity in a vertical line is found to be $\lambda f/h$ when the intensity at the central maximum is unity. With the value for the central intensity previously found, we now obtain the total linear intensity associated with λ to be

$$\frac{hl^2 \cos^2\beta}{f N^2 \lambda^3} R^2. \tag{2}$$

Changing the variable, the expression for R takes the form

$$\int_{x_0}^{x_0+N\lambda} \cos n(Vt-x)\,\phi(x)\,dx,$$

where x_0 is put for $Vt-\frac{1}{2}N\lambda$.

Write

$$A = \int_{x_0}^{x_0+N\lambda} \cos nx\,\phi(x)\,dx; \qquad B = \int_{x_0}^{x_0+N\lambda} \sin nx\,\phi(x)\,dx.$$

The mean value of R^2 is then equal to the mean value of

$$\tfrac{1}{2}(A^2+B^2).$$

A grating such as that to which the above equations apply forms two spectra and absorbs part of the light; we must now estimate what fraction of the incident beam is utilised to form the spectrum under consideration. For this purpose we imagine homogeneous light to fall on the grating, and put $\phi(x) = \cos nx$. The mean value of $\frac{1}{2}(A^2 + B^2)$ is then easily found to be $\frac{1}{8}N^2\lambda^2$. By substitution into (2) we find that the total linear intensity in the central line is now

$$\frac{hl^2 \cos^2\beta}{8f\lambda} R^2. \tag{3}$$

To either side of the principal maximum the intensity varies according to the law $\alpha^{-2}\sin^2\alpha$, where α is equal to $\pi\xi l\cos\beta/f\lambda$, ξ representing a distance measured at right angles to the spectroscopic line. The total energy measured in the focal plane of the telescope is obtained by multiplying (3) with $\alpha^{-2}\sin^2\alpha d\xi$, and integrating. This gives $\frac{1}{8}hl\cos\beta$.

If the incident light is normal to the grating, its total energy is $\frac{1}{2}hl$, the factor $\frac{1}{2}$ representing the fact that we have taken the average square of the velocity which is half the square of the greatest velocity as the measure of intensity. We conclude that $\frac{1}{4}\cos\beta$ is the fraction of light utilised to form the spectrum.

Taking account in (2) of this, we find that the type of spectroscope considered estimates the intensity of light passing through its central meridian as being

$$\frac{2hl^2\cos\beta}{fN^2\lambda^3}(A^2+B^2),$$

where for A^2 and B^2 their mean values are to be substituted.

To obtain the total light within a small angular distance $d\beta$, we must multiply by $Fd\beta$; as $Nd\lambda = l\cos\beta d\beta$, we find that the total energy within a range $d\lambda$ is

$$\frac{2hl}{N\lambda^3}(A^2+B^2)\,d\lambda.$$

If the total energy of the light incident on the grating is unity, the energy assigned by the grating to a range dn is therefore finally—

$$\frac{A^2+B^2}{\pi N\lambda}dn. \tag{4}$$

In the application of the periodogram it is more convenient to take the time as the independent variable. Defining therefore—

$$A = \int_{t_0}^{t_0+NT}\cos\kappa t\,\phi(t)\,dt, \qquad B = \int_{t_0}^{t_0+NT}\sin\kappa t\,\phi(t)\,dt, \tag{5}$$

(4) becomes equal to $\dfrac{A^2+B^2}{\pi NT}d\kappa$.

Leaving out the constant factor, I now define

$$S = (A^2+B^2)/NT$$

to be the ordinate of the periodogram. The definition differs from the previous one by the factor NT, which occurs in the denominator instead of its square.

The present definition is not only justified by the close optical analogy which has now been formally proved, but also by the resulting convenience. I have previously shown that, in the absence of any homogeneous

periodicities, the average of A^2+B^2 increases in proportion to the time interval, NT, which occurs in the limits of the integrals for A and B. It follows that for such variations, the ordinate of the periodogram as at present defined is independent of the time limits chosen. This is an advantage. On the other hand, the former definition gave directly the amplitude of the periodic variation when it was of an absolutely homogeneous character. For such homogeneous variation the present ordinate increases proportionally to the time interval chosen.

The optical analogy explains the reason of this, and gives its justification. When homogeneous light falls on an instrument of definite resolving power the light in the central meridian does not by itself give sufficient indication of the intensity of the incident light. It is only when correction has been made for the lateral spreading that the true intensity can be deduced, the correction depending on the resolving power. It is otherwise when the spectrum is continuous, for in that case the light lost by lateral spreading is replaced by that which properly belongs to the neighbouring wave-lengths. Hence, in this case, the intensity in the central meridian is a true measure of the intensity of the incident light.

It need hardly be pointed out how constant use is made of the fact that increased resolving power (*not* increased dispersion) brings out the homogeneous lines of a spectrum by increasing their intensity beyond that of the continuous background. It is correspondingly one of the principal advantages of the periodogram method that it gives a measure of the resolving power necessary to isolate a true homogeneous period from the irregular fluctuations.

Light is thrown on parts of the previous investigation by a formula given by Lord Rayleigh for the intensity to be assigned to the homogeneous components of a disturbance. If $\phi(x)$ be the velocity at any point of a linear disturbance, so that the total intensity is

$$\int_{-\infty}^{+\infty} \{\phi(x)\}^2\,dx,$$

Lord Rayleigh shows that the energy to be assigned to a range dn, where $n = 2\pi/\lambda$, is

$$(A^2+B^2)\,dn/\pi,$$

in which

$$A = \int_{-\infty}^{+\infty} \cos \kappa v\,\phi(v)\,dv, \qquad B = \int_{-\infty}^{+\infty} \sin \kappa v\,\phi(v)\,dv.$$

The average intensity spread over a certain length L may be estimated by taking v_0 and v_0+L as lower and upper limits of the integrals, and averaging the values obtained by a change of v_0. The energy per unit

length would then be found on dividing the expression in (5) by L. We arrive in this manner at equation (4).

I might have confined myself to this simple deduction had I not wished to lay stress on the equations for instrumental resolution. This seemed all the more desirable because for absolutely homogeneous radiation a definition of the periodogram based on the average intensity per unit length would fail. If a simple periodicity exists, its amplitude may easily be derived from the ordinate of the periodogram, for, if S be that ordinate, the amplitude is $2\,(S/NT)^{\frac{1}{2}}$.

In practical applications the function $\phi(t)$ will generally be given for successive intervals (α) of the time. The integrals occurring in (5) are then replaced by summations, unless a harmonic analyser is used. It is most convenient to write in this case

$$A = \sum_{s=0}^{s=(n-1)\alpha} \Phi_s \cos\left(\frac{2\pi}{n} s\right), \qquad B = \sum_{s=0}^{s=(n-1)\alpha} \Phi_s \sin\left(\frac{2\pi}{n} s\right), \tag{6}$$

$$S = (A^2 + B^2)\,\alpha^2/NT,$$

where ϕ_s represents the values which $\phi(t)$ takes at the successive times considered.

If we take p to be equal to the total number of separate values of $\phi(t)$ used in the calculations, we may put

$$S = (A^2 + B^2)\,\alpha/p. \tag{7}$$

If a harmonic analyser be used, and a b are the two Fourier coefficients,

$$S = \tfrac{1}{4}\,(a^2 + b^2)\,NT = \tfrac{1}{4}\,(a^2 + b^2)\,\alpha p,$$

where αp represents the complete time interval to which the Fourier analysis has been applied.

On Sun-spot Periodicities.—Preliminary Notice.

By Arthur Schuster, F.R.S.

(Received November 23,—Read December 7, 1905.)

In cases where it is necessary to separate true periodic changes from other variations, which during short periods of time often simulate periodicities, the method of the periodogram is at present the only one which can give definite results. In view of the importance of the questions connected with the changes in the frequency of the sun-spots, I have, therefore, undertaken the considerable labour of forming a complete periodogram of sun-spot variability as far as the data at my disposal allow me to do so. The following is a brief abstract of the results; the investigation will be presented shortly:—

The periodogram, as already explained, is the diagram representing the intensity of periodic variations as determined from the sum of the squares of the two Fourier coefficients belonging to each assumed period. This diagram represents for any regular or irregular change exactly what the energy diagram gives us for a luminous disturbance which is analysed by a spectroscope.

Periods of not more than three or four years' duration may be satisfactorily studied by means of the tables of sun-spot areas which are at our disposal. For periodic times which exceed four or five rotations of the sun, I have used the mean daily areas in each synodic rotation of the sun as collected and published by the Solar Physics Committee. The data reach back as far as 1832. For shorter periods we have to go back to the Greenwich measurements, which are published for each day since 1883.

In the investigations of periods longer than three years, I have used Wolf and Wolfer's sun-spot numbers, which are published for each month beginning with the year 1749, and based on a combination of spot and group counting.

The periodogram of the whole interval of 150 years showed, as was expected, a marked peak corresponding to the 11-year period, but it presented some features which rendered it desirable to investigate separately each of the two intervals of 75 years into which the whole range of time may be divided.

The result was surprising, for the two separate periodograms seem at first sight to have nothing in common. From 1750 to about 1825 the observations do not indicate any marked periodicity of approximately 11 years,

but two periods of 13⅗ and 9¼ years respectively. Since that time, on the contrary, the variations are well represented by an almost homogeneous period of 11·12 years. The apparent absence of the 11-year period in the last half of the eighteenth century cannot, in my opinion, be attributed to the unreliability of the observational data.

The method employed is specially suited to investigate possible effects of planetary configurations, which have often been supposed to exist. I have therefore specially investigated the rotation period of Jupiter and the synodic periods of revolution of the planets Venus and Mercury. My results do not show any trace of a planetary influence.

The most persistent period which I have found is one of 4·81 years' duration. It appears, with good agreement of phase, in Wolf's sun-spot numbers both before and after 1825. It also shows well in the direct measurements of sun-spot areas. The amplitude of this period seems to be about one-sixth of that of the main 11 years' periodicity.

An increased intensity is observed for periodic times of 5·625, 3·78, and 2·69 years, the importance of the periods being in descending order Multiplying these numbers by 2, 3, 4 respectively we obtain 10·76, 11·34, 11·25. These periodicities are, therefore, in all probability only sub-periods of the 11 years' variation.

The only remaining periodicity which is clearly indicated by the periodogram is one of 4·38 years' duration, but this does not occur in the observations previous to 1750, and only shows feebly in Wolf's series of figures as drawn from the observations of the last seventy years. Its real existence is, therefore, doubtful, though some prominent outbreaks during the last fifteen years may be associated with it.

Special care was devoted to the investigation of periodicities in the neighbourhood of 26 and 27 days. Some rise in the periodogram for periods approximating to the time of synodic solar rotation may be expected if an appreciable number of sun-spots live through several rotations. But apart from the fact that different circles of latitude have different periods of revolution, only a very broad band (in the optical sense) can be expected, as the number of rotations during which a sun-spot is observed very seldom exceeds three. Such a succession could only produce a general rise between the periods of 25 and 29 days; this rise does, indeed, seem to take place, but no definite periodicity has been discovered.

[*Note added December* 12.—Since writing the above, I have found strong evidence of the reality of a cycle having a periodic time of about 8·37 years. This evidence, which seems to me convincing, is briefly as follows:—If by

means of the table published by the Solar Physics Committee, giving the mean daily areas of spots for each year since 1832, the average value for the eleven-year cycle in each year is calculated and deducted from each entry, we obtain a series of numbers which may be taken to represent the sun-spot variation with the eleven-year period eliminated. This series shows decided maxima, which took place in the years 1836, 1845, 1853, 1862, and 1870, the intervals being alternately nine and eight years. The periodogram based on Wolf's numbers for the total interval 1749 to 1900 shows a decided maximum for a periodicity of 8·25 years. Provisionally accepting this period and taking the phase as obtained from the interval 1749 to 1826, we may forecast the maxima for the subsequent interval. We thus obtain: 1836·3; 1844·7; 1852·9; 1861·2; 1869·4 in almost exact agreement with the above. Taking into account the apparent shift of phase which is noticed when the observations of the first and second portions of the complete interval are taken into account, the more exact time of the period seems to be 8·38.

A further confirmation of this period may be obtained by means of an empirical criterion which I have found useful. In a true periodicity, the main sine or cosine variation is in the spot cycles accompanied by the higher harmonic which has a period of half the length of the main period. This harmonic is distinguished by a rise in the periodograph curve. Such a rise where it exists gives increased probability to the real existence of the period considered. In the case of fictitious periods, on the other hand, I have generally found that when the intensity of a certain period is exceptionally great, the whole of the accidental variations being as it were concentrated into it, the semi-period shows an exceptionally low intensity. As regards the cycle under consideration, it is therefore significant that the periodogram has a maximum for the period of 4·125 years in both the intervals 1750 to 1826 and 1826 to 1900.

In tracing backwards this new period as well as the one which is generally admitted to have a periodic time of a little over 11 years, the conclusion has been forced upon me that while the times of the maxima seem to occur with almost astronomical accuracy, the intensity of action is subject to great variations. This conclusion I believe to give the key to the explanation of the great irregularities which are observed in the succession of sun-spot cycles. While in the period 1750 to 1826 the eleven-year period does not appreciably affect the periodogram, it was not totally absent, for what there remains of it fits in well with the phase of the great eleven-year cycle which has been observed since 1826. I have approached this question without preconceived opinions, and my first impression on looking at the periodogram tended towards a denial of fixed periods extending over long ranges of time,

but gradually the conviction has forced itself upon me that there are a number of perfectly definite periodicities, having the peculiarity that for a certain number of cycles they are effective and then cease to be active. Their real existence is proved by the fact that whenever they reappear after a period of inactivity, they form, without change of phase, a continuation of the former periodicities. The phenomenon reminds one to some extent of the beats of sound, but the evidence at present is against an explanation founded on the theory of interference of two nearly equal oscillations.

The periodogram for the total interval 1825 to 1900 shows a rise for a periodicity of about 13·5 years. Looking at the records of sun-spot maxima since 1600, we find three examples of two successive maxima being separated by an interval of about 13·5 years. They are: 1626·0 to 1639·5; 1816·4 to 1829·9; and 1870·6 to 1883·9. Taking the interval between 1639·5 and 1816·4, we find it to be 13 times 13·61, while the interval between 1829·9 and 1870·6 is three times 13·57. This would tend to show that the maxima in question belong to a periodicity of about 13·57 years, which has extended right through two and a-half centuries. It is specially to be noted that no observation previous to 1749 was used in determining the periodogram, so that the coincidence of the maxima as far back as 1626 is remarkable. Without wishing to attach too great a weight to what may quite possibly be only a numerical accident, I think that as a confirmation of conclusions arrived at independently, the coincidence deserves notice.

The periodicities which so far have been traced with a considerable amount of probability were found to have periodic times of 4·81, 8·38, and 11·125 years. There are indications that the first of these periods is somewhat less than the number given, and at one time I had put it down as low as 4·75, but the above were actually the times directly determined, and I adopt them so as to avoid suspicion of having in any way helped by personal bias to improve the following remarkable relationship between the numbers. That there was a connection between the numbers was only discovered after the first draft of my paper had been written out giving the above figures as highly probable periodicities. Taking frequencies into consideration in place of periodic times, we are led to consider reciprocals, and thus find—

$$\begin{aligned} (11{\cdot}125)^{-1} &= 0{\cdot}08989 \\ (8{\cdot}38)^{-1} &= 0{\cdot}11933 \\ \text{Sum} = (4{\cdot}78)^{-1} &= 0{\cdot}20922 \end{aligned}$$

Hence the sum of the frequencies of two of the periods agrees, within the possible errors, with the frequency of the third period.

But it is also found that the two first numbers are very nearly in the ratio

of three to four, so that we may also express the three periodic times as sub-periods of 33·375 years; thus—

$$\tfrac{1}{3} \times 33{\cdot}375 = 11{\cdot}125$$
$$\tfrac{1}{4} \times 33{\cdot}375 = 8{\cdot}344$$
$$\tfrac{1}{7} \times 33{\cdot}375 = 4{\cdot}768$$

How far this connection is accurate or approximate, it is impossible to say at present, but the fact that the three periods which have been traced with a considerable degree of certainty should also bear a remarkably simple relationship to each other is worthy of note. If we accept a period twice as long as that given above, we might account for other periodicities of which at present the times are only approximately determined; thus $\tfrac{1}{5} \times 66{\cdot}75$ would lead us to 13·34, in fair agreement with the period of 13·57 years which has been mentioned above. But for the present I do not wish to lay any stress on this.

I have confined the discussion to the statistical problem, but could not help giving some thought to the possibilities of explanation. It is not difficult to form a theory which should account for the peculiarity of interruption in the succession of cycles which has been found to be characteristic of the sun-spot periods. A reason may also be found for the first relationship between the cycles. But the harmonic dependence on a long period is more difficult to account for.]

The Combination of Hydrogen and Oxygen in Contact with Hot Surfaces.

By William A. Bone, D.Sc., F.R.S., and Richard V. Wheeler, B.Sc.

(Received November 14,—Read December 7, 1905.)

(Abstract.)

The authors have made a systematic investigation of the slow combination of hydrogen and oxygen in contact with various heated surfaces, including porous porcelain, magnesia, metals such as gold, silver, platinum and nickel, and reducible oxides such as ferric oxide, nickel oxide, and copper oxide.

In each case the moist gaseous mixture was circulated at a uniform speed over the surface, which was maintained at a constant temperature in the combustion tube of the "circulation apparatus" employed by the authors in their researches on the slow combustion of hydrocarbons.

Since the steam produced was rapidly removed by condensation from the sphere of action, the rate of combination was measured by observing the pressure fall in the apparatus during successive time intervals.

The results prove conclusively that in no case does the rate of combination depend on the "order" of the reaction, as was asserted some years ago by Bodenstein* (in the case of porous porcelain), nor yet is it governed by diffusion factors, as suggested by Nernst.† Equally certain, also, is it that the process cannot be explained by any purely chemical theory, such, for example, as the view that it involves a rapidly alternating series of oxidations and reductions of the catalysing material.

The catalysing power of a new surface usually increases up to a steady maximum when successive charges of electrolytic gas are circulated over it, and after the attainment of this steady state, the rate of combination for normal electrolytic gas is always directly proportional to the pressure. That is to say, the velocity curve for electrolytic gas is always of a "monomolecular" type.

When one or other of the reacting gases is present in excess, the rate of combination is nearly proportional to the partial pressure of the hydrogen. This applies to all the surfaces examined, except silver and copper oxide. In the case of silver, the rate is proportional to the partial pressure of the hydrogen up to a certain condition of maximum "hydrogenation" of the surface, after which it is governed largely by the oxygen pressure. In

* 'Zeit. Phys. Chem.,' vol. 29, p. 665, 1899.

† 'Zeit. Phys. Chem.,' vol. 47, p. 52, 1904.

the case of copper oxide, the rate of combination depends mainly on the partial pressure of the oxygen.

The catalysing power of porcelain, magnesia, and the metallic surfaces examined, can be stimulated, often in a high degree, by previous exposure to hydrogen at moderately high temperatures, and all these surfaces have the power of occluding hydrogen at dull red heat.

The results of the research as a whole prove that, except in the case of copper oxide, hydrogen plays an all important *rôle* in the catalytic process, being rendered "active" by association with the surface. In the majority of cases the hydrogen is merely "occluded" or "condensed" by the surface, but in the case of silver, there is evidence of a more intimate association, such as the formation of an unstable hydride at the surface.

In the case of copper oxide, the catalytic process depends primarily on the condensation of a film of "active" oxygen on the surface; so far from the surface taking any *direct* part in the formation of steam, it is actually protected by the film of condensed oxygen from the attacks of the hydrogen, which would otherwise energetically reduce it.

Experiments on the Chemical Behaviour of Argon and Helium.

By W. TERNENT COOKE, D.Sc.

(Communicated by Sir William Ramsay, K.C.B., F.R.S. Received December 7, 1905,—Read January 25, 1906.)

Historical and Introductory.—The discoverers of argon, Rayleigh and Ramsay, showed* that when subjected to very severe chemical treatment the gas failed to combine with any other element. Later on Collie and Ramsay also showed† that helium resembled argon in being chemically inactive. Moissan‡ and Berthelot§ both carried out experiments with argon, but neither obtained real proof of any power of argon to enter into chemical combination.

It has always been assumed that if union of argon or helium with any other element occurred the action would be strongly endothermic, and experiments have always been framed in such a way as to impart to the system a plentiful supply of energy.

There being a sufficient quantity of both gases at disposal, it was deemed desirable to make further experiments on the chemical behaviour of argon and helium, and to work at temperatures higher than those which hitherto had usually been employed. Then, again, it was desired to obtain, if possible, evidences of partial combination, and to this end experiments on the vapour densities of various elements in both gases, at temperatures between 1200° and 1300° C., were carried out.

When a known weight of a substance is vaporised in a given volume of gas, which is chemically inert towards the substance, we can calculate the density of the vapour, if we know the final temperature and pressure of the gaseous mixture. If, however, the gas of the substance combines totally, the resulting compound will have, generally, a greater density than that of the vapour of the substance. High values, then, for the density may be taken as indicative of chemical combination, even if the combination be but partial. The method used in this work was to obtain values for the densities of various elements in inert gases, and to compare these values with those found in the same apparatus when argon or helium was the gas used. High

* 'Phil. Trans.,' A, 1895, p. 231.

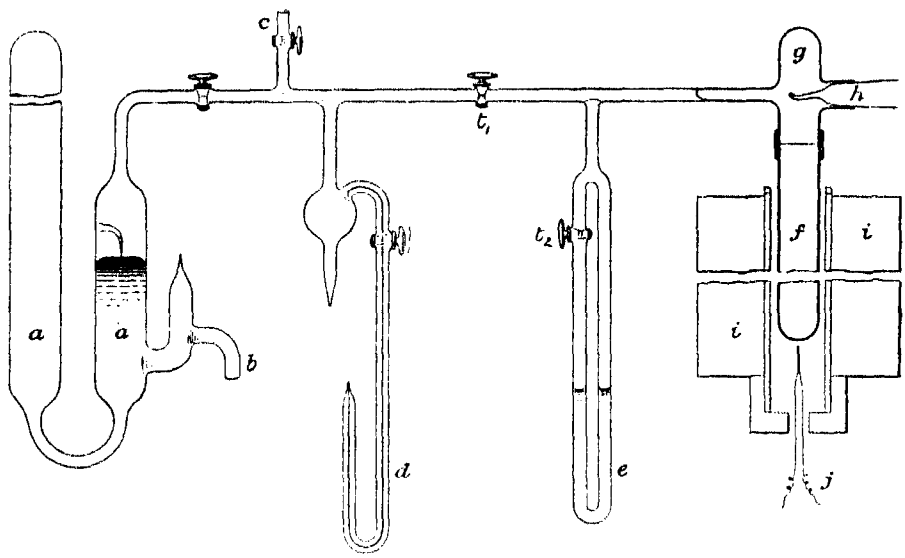
† 'Roy. Soc. Proc.,' vol. 60, p. 53.

‡ 'Compt. Rend.,' vol. 120, p. 966.

§ 'Compt. Rend.,' vol. 120, pp. 581, 1316; and vol. 129, pp. 71, 378.

values for the densities were considered as evidence of the occurrence of chemical combination.*

The Apparatus.—Heat was supplied by means of an electrical resistance oven, and temperatures were determined by means of a thermocouple of platinum—rhodioplatinum—which was calibrated by the method of fixed points. The main portions of the apparatus, which is essentially a Victor Meyer apparatus, is shown in the sketch :—



a is a constant volume manometer, connected at *b* to a movable reservoir of mercury.
The tubing at *c* leads to a Töpler pump.
d is a capillary U-tube, used for introducing gas into the apparatus.
e is a differential manometer.
f is a silica glass tube, 25 cm. long, 12 mm. internal diameter, and about 1·5 mm. thick in the walls ; 15 to 16 cm. of its length projected into the oven.
g is a glass head-piece, which was secured to the silica tube by means of thick rubber tubing. This rubber joint was surrounded with mercury.
h is a stopper working in a ground glass joint. The stopper is shaped like a spoon at one end, and serves to hold the substance previous to its being dropped into the silica tube.
i is an electric resistance oven.
j is a thermo-couple.
t_1 and t_2 are taps.

Method of Working.—Suppose all the air has been pumped out of the apparatus, that the temperature is sufficiently high, and that a weighed

* *Note by Sir William Ramsay.*—It has been noticed ever since the discovery of argon that metals such as platinum, magnesium, and aluminium, used as negative electrodes in

amount of substance is in the spoon, ready to be dropped. Helium, say, is admitted into the apparatus through the capillary U-tube, *d*, and the pressure of the gas obtained by means of the absolute manometer. The tap, t_1, is closed, the level of the liquid in the left limb of the differential manometer noted, the tap, t_2, closed, and the contents of the spoon tipped out. The level of the liquid in the left limb of the differential manometer rises, and the reading of this is taken. The temperature is then immediately read. All the necessary readings having been thus ascertained, the helium is pumped out, and the oven allowed to cool.

Calculation of the Density.—It will be seen that the volume of the vapour is determined indirectly, *i.e.*, by noting the increase of pressure it occasions. To find the volume, consider first the differential manometer. At the beginning of the experiment the pressure, p_1, is equal in both limbs. Call V_1 the volume of the closed, V_2 the volume of the open limb. When the substance vaporises the alteration of the level of the liquid in both limbs is equal. The volume of the closed limb becomes V_3, and the pressure p_3, where $p_3 = p_1V_1/V_3$. In the open limb the pressure becomes p_4, and the volume V_4. Now $p_4 = p_3+2h$, where h is the displacement of the level of the liquid, supposing it to be mercury. If, now, the mass of the gaseous contents of the open limb had remained constant, it would have occupied, under the pressure p_4, a volume V_2p_1/p_4 instead of V_4. The volume of the

a Plücker tube, "splashed" much more readily in that gas than in oxygen, nitrogen, or hydrogen, at low pressures. After the discovery of the other inert gases, this observation was a matter of almost daily occurrence. An explanation of this remarkable phenomenon may be found in the supposition that the energy imparted to the gas at its surface of contact with the electrode is sufficient to cause chemical combination between the two in that region. On leaving the electrode, however, the supposed endothermic compound may be imagined to dissociate, and the gas may be liberated, with simultaneous deposit of the metal on the walls of the tube. Similar experiments with zinc, cadmium, antimony, and mercury have shown that they, too, when made the cathode in an atmosphere of one of these gases, volatilise much more easily than in nitrogen or hydrogen. It has also been often noticed that on filling a vacuum tube with an indifferent gas, in which operation the gas has stood in contact with mercury, the spectrum of mercury is seen in the vacuum tube more frequently than when diatomic gases are submitted to the same treatment. These facts afford presumptive evidence that chemical combination occurs to a limited extent between metals and the indifferent gases.

Again, in repeating Berthelot's experiment of submitting argon and helium to the silent discharge in contact with benzene vapour and mercury, it was observed that, so long as nitrogen was present, the glow in the annular space between the electrodes had the usual faint violet colour; but as soon as all nitrogen had entered into combination, the tube glowed with a brilliant green light. Examined with the spectroscope, this showed only the lines of mercury. Here again there is presumption that absorption of energy causes combination between the inert gas and mercury, forming a compound more volatile than mercury, but decomposing almost as soon as formed.

vapour at this pressure is, therefore, $V_4-V_2p_1/p_4$. Calculating for a pressure p_1, we have for the volume of the vapour V_v

$$V_v = \frac{p_4V_4}{p_1} - V_2. \tag{A}$$

By weighing the mercury contained between any two points in the manometer tube it was found that every millimetre of length corresponded to a volume of 0·0093 c.c. Hence,

$$V_3 = (V_1 - 0{\cdot}0093h),$$
$$V_4 = (V_2 + 0{\cdot}0093h),$$

h being expressed in millimetres.

Again

$$p_3 = \frac{V_1p_1}{V_3} = \frac{V_1p_1}{(V_1-0{\cdot}0093h)}$$

and

$$p_4 = (p_3+2h) = \frac{V_1p_1}{(V_1-0{\cdot}0093h)} + 2h.$$

Inserting these values into equation A, we get

$$V_v = \frac{[V_1p_1/(V_1-0{\cdot}0093h)+2h][V_2+0{\cdot}0093h]}{p_1} - V_2. \tag{B}$$

V_1 was determined by finding the weight of mercury required to fill the space, and V_2 by measuring the amount of dry air contained in that volume of the apparatus.

We have now the volume in cubic centimetres of the vapour given by a known weight of substance, the volume being determined for a pressure p_1 and a temperature $t°$ C. The density of the vapour, taking H = 1, is given by the equation

$$D = \frac{760 \times 1000}{0{\cdot}899 \times 273} \times \frac{g(t+273)}{V_vp_1}, \tag{C}$$

where g = weight of substance in grammes and 0·0899 gramme is the weight of 1 litre of hydrogen at N.T.P.

When the apparatus was used to determine the vapour density of zinc in nitrogen, the values found were from three to four times too great. An error in this direction is to be expected, since the relative dimensions of the various parts of the apparatus were unsuitable for an apparatus intended to give absolute values. The volume of the heated part should be many times greater than that of the cooler part, whereas in the present case about one-third of the volume, that is the volume of the open limb of the differential manometer, was at the highest temperature. Moreover, diffusion and mixing is very rapid at high temperatures. Since, however, in the

present case, differential results only are required, comparative figures were obtained by determining the density of the substance used in some indifferent gas besides in argon and helium. For the sake of convenience, figures are reduced to absolute values. The reducing factor is obtained by calibrating the apparatus, using as a standard the mean of the values found for zinc in an atmosphere of nitrogen. The mean of five determinations gave the value 111. This value multiplied by 0·293 gives 32·5, which is the vapour density of zinc when hydrogen is taken as unity.* Hence, to reduce to its absolute value, the density of a vapour was multiplied by 0·293.

There is another fact to be considered. In a light gas like helium, diffusion is much more rapid than in argon or nitrogen, and higher values for the density are likely to be obtained in the lighter gas. For the sake of a juster comparison, a series of determinations was made in hydrogen, if the nature of the substance allowed. Hence, in making comparisons, values found in argon are compared with those found in nitrogen, and values in helium with those in hydrogen.

Materials Used.—The argon and helium were carefully purified in the usual way by passage over heated lime and magnesium mixture, and also over heated copper oxide. Spectroscopic examination showed that each gas was practically pure, a trace of hydrogen only being present.

"Atmospheric" nitrogen was used; it was obtained by burning phosphorus in dry air, and finally subliming part of the phosphorus in the nitrogen left.

Hydrogen was made from electrolytic zinc and pure sulphuric acid.

All the gases used were thoroughly dried before use by passage over phosphorus pentoxide.

Of the substances whose vapour densities were required, ordinary samples were taken, the purest at hand being selected. The substances used were zinc, cadmium, mercury, sulphur, selenium, and arsenic. The number of elements which could be used was limited. The temperatures were not high enough to vaporise heavy metals such as lead or tin. Antimony, bismuth, and tellurium just begin to vaporise.† Bismuth, too, attacks silica slightly. Phosphorus presented experimental difficulties which could not be overcome. The use of potassium, etc., calcium, etc., and magnesium is out of the question, since heated silica is at once attacked by them. The size of the apparatus enabled only very small amounts of the substances to be used, and less than 3 milligrammes were sufficient. Weighings were made by the method of swings. Weights were calculated to 0·01 milligramme, and are accurate to within 2 per cent.

* V. Meyer, 'Ber.,' vol. 19, p. 3295.

† *Cf.* Blitz and Meyer, 'Zeit. Phys. Chem.,' vol. 4, pp. 259, 263.

Table of Results.

Weight in milli-grammes.	Tempera-ture in ° C.	Density found.	Density corrected.
Zinc in nitrogen (standard).			
1 ·54	1270	117 ·1	—
1 ·25	1284	106 ·5	—
1 ·09	1271	107 ·8	—
1 ·45	1308	110 ·7	—
1 ·54	1294	112 ·8	—
		111 ·0	32 ·5
Zinc in argon.			
1 ·51	1285	130 ·0	38 ·1
1 ·63	1299	132 ·0	38 ·7
1 ·33	1278	119 ·0	34 ·9
1 ·63	1278	111 ·0	32 ·5
2 ·28	1268	130 ·0	38 ·1
		124 ·5	36 ·4
Zinc in hydrogen.			
1 ·13	1250	114 ·2	33 ·4
1 ·27	1280	107 ·3	31 ·4
1 ·82	1310	118 ·2	34 ·6
1 ·77	1268	132 ·2	39 ·6
1 ·67	1276	123 ·6	36 ·2
2 ·15	1276	148 ·0	44 ·3
		124 ·4	36 ·4
Zinc in helium.			
1 ·37	1284	125 ·8	36 ·9
1 ·46	1280	121 ·0	35 ·4
1 ·46	1277	129 ·0	37 ·8
1 ·63	1267	120 ·1	35 ·2
1 ·53	1283	114 ·5	33 ·5
		122 ·1	35 ·8
Cadmium in nitrogen.			
1 ·93	1281	191 ·2	56 ·0
1 ·23	1275	183 ·8	53 ·8
2 ·16	1196	198 ·4	58 ·1
		191 ·1	56 ·0
Cadmium in argon.			
1 ·64	1292	189 ·8	55 ·6
2 ·04	1289	186 ·6	54 ·7
2 ·6	1237	178 ·2	52 ·2
		184 ·86	54 ·2
Cadmium in hydrogen.			
1 ·81	1260	169 ·1	49 ·5
2 ·1	1252	180 ·9	53 ·0
1 ·97	1234	162 ·1	47 ·5
		170 ·7	49 ·9
Cadmium in helium.			
1 ·68	1288	192 ·7	56 ·5
1 ·77	1273	198 ·9	58 ·3
2 ·16	1260	184 ·8	54 ·1
2 ·07	1266	189 ·8	55 ·6
		191 ·5	56 ·1
Mercury in nitrogen.			
2 ·81	1291	370 ·55	108 ·67
2 ·41	1265	389 ·95	114 ·26
2 ·38	1190	312 ·36	91 ·52
2 ·35	1288	335 ·46	98 ·16
		352 ·1	103 ·2
Mercury in argon.			
1 ·86	1308	409 ·9	120 ·1
2 ·4	1287	324 ·4	95 ·0
2 ·04	1287	482 ·3	141 ·3
2 ·49	1300	358 ·0	104 ·9
		393 ·6	115 ·3
Mercury in hydrogen.			
1 ·73	1266	380 ·1	111 ·4
2 ·35	1278	340 ·9	99 ·9
		360 ·5	105 ·6

Table of Results—*continued.*

Weight in milli-grammes.	Tempera-ture in ° C.	Density found.	Density corrected.
Mercury in helium.			
2·07	1286	356·7	104·5
2·07	1284	352·4	103·2
3·02	1244	371·6	108·9
2·45	1278	335·9	98·4
4·25	1246	384·4	112·6
1·945	1282	463·7	135·9
		377·5	110·6
Arsenic in nitrogen.			
1·82	1286	332·15	97·3
2·41	1280	281·1	82·4
2·205	1271	362·95	106·3
1·71	1295	298·7	87·5
3·02	1203	425·4	124·6
		340·1	99·65
Arsenic in argon.			
1·92	1263	341·43	100·0
1·88	1291	325·77	95·5
		333·6	97·7
Arsenic in helium.			
1·96	1288	343·0	100·5
1·92	1183	322·7	94·5
2·215	1202	350·2	102·6
2·32	1261	339·4	99·4
		338·3	99·1
Sulphur in nitrogen.			
1·57	1292	101·8	29·8
1·4	1230	101·5	29·7
1·81	1291	160·7	47·1
2·2	1246	106·7	31·2
		117·7	34·5
Sulphur in argon.			
1·44	1303	114·9	33·7
1·3	1286	114·2	33·4
		114·5	33·55
Sulphur in helium.			
1·4	1276	107·6	31·5
1·236	1284	102·7	30·1
1·41	1244	104·9	30·8
1·52	1300	113·6	33·3
1·68	1250	146·3	42·9
		115·0	33·7
Selenium in nitrogen.			
1·92	1277	227·6	66·7
2·49	1249	239·6	70·2
1·925	1273	230·6	67·6
		232·6	68·1
Selenium in argon.			
1·63	1275	245·3	71·9
2·1	1278	252·3	73·9
1·88	1288	227·4	66·6
		241·7	70·8
Selenium in helium.			
2·01	1302	275·1	80·6
2·1	1286	209·0	60·3
2·25	1280	251·4	73·6
		245·2	71·8

Discussion of Results.—Taking the elements separately and comparing the densities in argon with those in nitrogen, and the densities in helium with those in hydrogen, it will be seen that there is a distinct tendency of zinc

and argon to combine, the density being 12 per cent. higher in argon than in nitrogen. With helium there is no tendency to combine.

Cadmium and argon show no tendency to combine, but in helium the density is 12·4 per cent. higher than in hydrogen. The values for mercury are very irregular. The irregularity is due mainly to the fact that when the globule of mercury, which is very small, is placed in position for dropping, it is at a temperature high enough to melt sulphur, and for some time is exposed to a vacuum. Under these conditions it undoubtedly suffers loss in weight. The mean values show that there is a tendency for the metal to combine with argon and with helium.

In the case of arsenic, of sulphur, and of selenium, it is, of course, impossible to obtain values in hydrogen, since chemical combination takes place. Arsenic and sulphur show no tendency to combine with either gas, while selenium exhibits a slight tendency to combine with both.

I desire to tender my best thanks to Professor Sir William Ramsay, who suggested the research to me, and besides kindly placing at my disposal the argon and helium necessary for the experiments, gave me every encouragement in the work.

The Determination of the Osmotic Pressures of Solutions by the Measurement of their Vapour Pressures.

By the EARL OF BERKELEY and E. G. J. HARTLEY, B.A. (Oxon.).

(Communicated by W. C. D. Whetham, M.A., F.R.S. Received November 11,—Read December 7, 1905.)

In May, 1903, we described,* in a preliminary paper, some experiments by which we had measured directly the osmotic pressures of strong solutions of cane sugar in water. Since then a repetition of the experiments with an improved apparatus has been carried out, the results of which we hope to publish very shortly.

As these results depart widely from the theoretical gas laws, it was deemed advisable to obtain the osmotic pressures of the same solutions by an independent method; and an experimental investigation of the well known connection between the "lowering" of the vapour pressure of a solution and its osmotic pressure seemed suitable.

After some experiments with a dew point method, the dynamical method due to Ostwald and Walker was selected, as it promised to be a rapid one and to be capable of giving results both with the solutions, the osmotic pressures of which we had already measured, and those where the osmotic pressures are higher than direct measurements can reach; also it is applicable to solutions of substances for which no semipermeable membranes have yet been found.

The method is described by Ostwald† as follows: "Two Liebig's potash bulbs containing the solution, and one containing the water, are connected with each other. The last is weighed, and is in its turn connected with a U-tube containing pumice soaked in sulphuric acid. A current of air is drawn through the apparatus. The air first saturates itself up to the vapour pressure of the solution then takes up from the water the quantity of vapour necessary for complete saturation, all of which it finally gives up to the sulphuric acid. The loss of weight of the water vessel is to the increase of weight of the sulphuric acid as the difference between the vapour pressure of the solution and of pure water is to the vapour pressure of pure water."

It was thought advisable to modify the general arrangement so as to obtain a closer knowledge of the best conditions for carrying out an experiment. With this in view, the air, before entering the solution, was completely dried by

* 'Roy. Soc. Proc.,' vol. 73, p. 436.

† Ostwald, 'Physical Chem. Measurements,' p. 188, Walker's translation, 1894.

passing through sulphuric acid, but the remainder of the train of vessels corresponded to the arrangement described above. Then by weighing the two vessels containing the solution, three objects were attained: (1) The weight lost by the solution is a direct measure of its vapour pressure, and consequently this loss in conjunction with the loss of weight of the water vessel gives a value for the ratio of the two vapour pressures to one another; (2) it is evident that the sum of the losses in weight of the solution and water vessels should equal the gain in weight in the vessel containing the sulphuric acid. This, it was hoped, would give a test as to the value of the particular experiment. It was found, however, that a small quantity of water always condensed in the junction leading from the water vessel to the sulphuric acid, and was therefore lost;* hence the gain in weight of the sulphuric acid was not used in the calculations; (3) by weighing the second vessel containing the solution an insight is afforded into the question as to whether the air, on emerging from the first vessel, was saturated up to the vapour pressure of the solution or not. We would draw attention to this, as it seems an important guide to the weight to be attributed to the experiment.

Testing the Efficacy of the Method.—This second vessel always lost weight, and numerous experiments were carried out in the endeavour to find the cause. The loss occurred in spite of alterations in the rate of bubbling, or in the forms of the absorption tubes. Eventually the explanation was found in the following fact.†

If air be passed through two absorption bulbs, which are filled with water, and connected together, the bubbles, owing to the difference in hydrostatic pressure, will increase in volume during their passage. Consequently, the air, although saturated on leaving the first bulb, will take up yet more water vapour from the second.

Another error which may occur with the bubbling method, is that particles of fine spray may be given off and thus cause loss of weight. That this occurs was shown by rapidly bubbling air in one case through sulphuric acid into a barium chloride solution, and in the other through a barium chloride solution into one of silver nitrate; in both cases there was a precipitate.

Modification of the Method.—The bubbling method was therefore discarded, and it was sought to avoid these sources of error by passing the air *over* the liquids instead of through them. Flat glass spirals were made out of $\frac{3}{8}$-inch tubing. Each spiral was about 30 inches long, and to increase the surface, they were nearly filled with glass beads.

* We hope to overcome this difficulty in the course of further experiments.

† See 'Nature,' July, 1905.

The loss of weight of the spiral containing the second solution was now considerably less than before, and with two spirals containing water there was no loss in the second. These spirals were discarded, however, owing to the unaccountable breakages which occurred, and also because it was thought that the loss in the second spiral might be due to the surface of the solution in the first spiral having become concentrated through loss of water, such loss not being compensated for by the stirring which necessarily occurs in the bubbling method.

Final Form of Apparatus.—The vessel which seems to satisfy all the requirements is shown in figs. 1, 2, and 3; the first figure is a plan, the second and third are side and end elevations respectively.

FIG. 1.

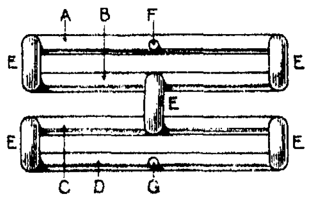


FIG. 2.

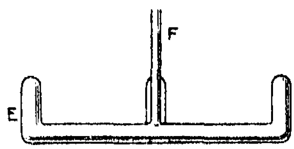
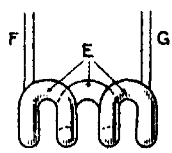


FIG. 3.



A, B, C, and D are glass tubes of about 1·5 cm. diameter and 20 cm. long; these are joined together, in the manner shown in the plan at E, by inverted U-tubes about 5 cm. high. F and G are the inlet and exit tubes; they are about 8 cm. long and are 0·75 cm. in diameter. The vessel is filled to about a third of its capacity with the liquid, and it will easily be seen that on slightly raising one end or the other, a clear passage for the air is obtained in that end. Thus by supporting the vessel on a platform, and oscillating the latter about an axis parallel to the line joining F G, the liquid in each of the four branches is caused to flow from one end to the other. This flow keeps the solution stirred, and also periodically wets the ends of the branches, and at the same time the contents of the branches are prevented from mixing by the air entrapped in the turned up ends.

It was considered that the fact last mentioned is of some importance, for if the rate of passage of the air be slow enough, it will practically be saturated up to the vapour pressure of the solution when it has passed through the first three branches. The solution in the fourth branch then remains at the same concentration throughout the experiment, and the air on emerging from this branch is saturated up to the vapour pressure of the original solution.

As a further help to the attainment of complete saturation, the surface

of liquid in contact with the air was increased by filling the last branch of each vessel with platinum foil rolled up into small tubes about 1¼ cm. long and ¼ cm. in diameter.

To test this apparatus, air was drawn through two of the vessels containing sodium chloride solution (75 grammes in 250 c.c.), which were joined together and placed on the oscillating platform in a water bath; two experiments were made, and in neither was the change in weight of the second vessel greater than 0·0003 gramme; the rate of passage of the air was slightly faster than that used in a determination of the lowering of the vapour pressure, and it was passed through for three days.

The Joints.—Considerable difficulty was experienced in finding a satisfactory method of joining two vessels to one another; it may, therefore, be of use to mention some of the forms of joint which failed.

By reason of the long length of time an experiment lasted, it was essential that all the vessels of the train should be as nearly as possible at the same temperature. They were, therefore, placed in a water bath; consequently the joints had to withstand the pressure due to the height of water above them, besides being impermeable to water vapour. Mr. Shaw* has shown that rubber is slightly permeable to water vapour, so plain rubber tubing was not suitable.

FIG. 4.

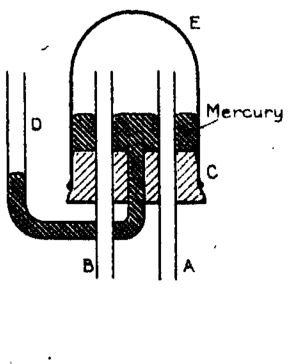
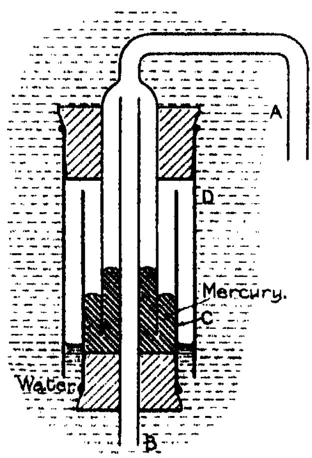


FIG. 5.



A joint shown in fig. 4 was tried. It consisted of an indiarubber plug, C, which served to support a glass dome, E, and which was perforated with three holes; two of these served for the passage of the tubes, A, B, leading to the two vessels to be joined to one another, and the third gave passage

* 'Phil. Trans.,' vol. 179, p. 97.

to a U-tube, D, through which mercury could be introduced so as to cover the top of the plug. This form of joint was given up owing to the difficulty of manipulation.

Another joint tried is shown in fig. 5. The tubes to be joined are A and B. A turns over as shown and widens out so as to overlap the end of B. By means of an indiarubber plug, a mercury cup, C, is supported on B, and enough mercury to close the bottom of A is placed in the cup; another tube, D, is secured to A by a plug at such a height that it overlaps the whole joint, and thus serves to keep the water of the bath off the mercury. This joint was discarded, because it was found that the alternate compression and expansion of the air inside D, due to the oscillation of the platform, caused water to be deposited on the mercury, and it was feared that some of this moisture might creep round the bottom of A.

FIG. 6.

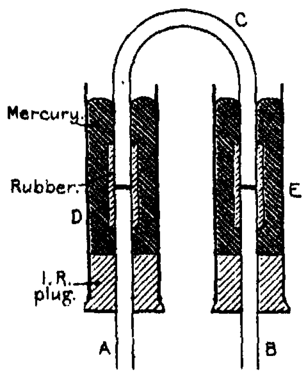
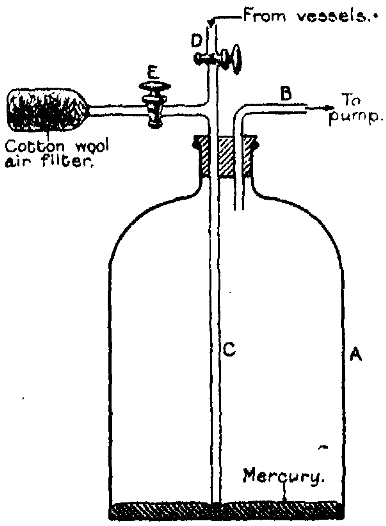


FIG. 7.



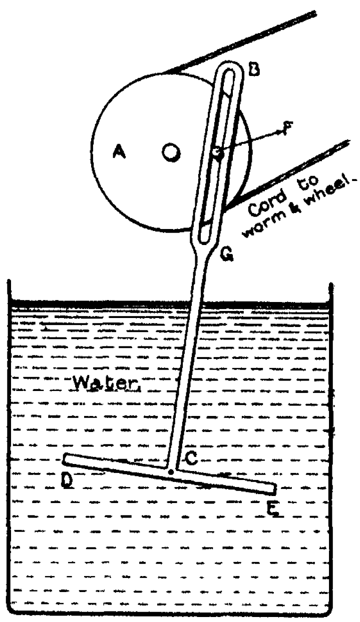
The joint which seemed the most satisfactory is shown in fig. 6. The two tubes to be joined are A and B. C is an inverted U-tube whose ends butt up against A and B, and are secured there by rubber tubing. Both joints are made tight by slipping the glass sleeves, D and E, which are carried by rubber plugs, up to a height such that the mercury in them will cover the rubber tubing to a depth of about 1 cm.

The Bath.—A large copper bath 21″ × 14″ × 18″ deep was used; it was filled with water, whose temperature was kept constant by means of a thermostat. The temperature in the later experiments did not vary by more than 3/100 of a degree during the whole run of an experiment. The bath

water was kept vigorously stirred night and day by a large stirrer actuated by the laboratory shafting.

The Air Current.—The air was drawn through the train of vessels by a double acting Fleuss pump, which was driven by the laboratory shafting. To obtain as steady a stream of air as possible, the pump was connected to a large air reservoir as shown in fig. 7. A is a 10-litre glass bottle which is connected to the pump by the tube B. The tube C, which connects to the vessels through the tap D, dips under a shallow layer of mercury at the bottom of A, thus forming a kind of non-return valve. The tap E gives connection to the atmosphere, and the air current is regulated by adjusting the two taps.

FIG. 8.



The Oscillating System.—The laboratory shafting also drove a worm and wheel, and the latter, by means of a pulley cord, caused the 9-inch wheel A (see fig. 8) to revolve once in six minutes.

The platform, D E, on which the vessels stand, is rigidly connected to the arm BC, and is pivoted about C (supports not shown). A pin, F, revolving with A, and working up and down the slot, B G, communicates the requisite motion to the arm. The dimensions of the various parts are such as to cause B to travel through an arc of 20 degrees.

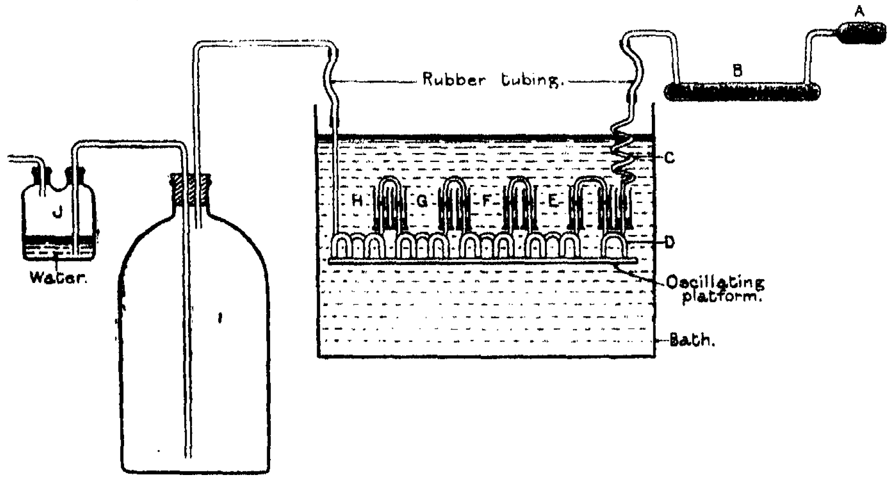
The Operation of Determining the "Lowering" of Vapour Pressure.—Fig. 9 is a diagram representing the assembled apparatus. The air enters at A, where it is filtered through cotton-wool, it then passes into the tube, B, which is nearly filled with beads moistened with sulphuric acid. On emerging from B it is led by rubber tubing into the glass spiral, C, where it takes up the temperature of the bath, and then into the sulphuric acid vessel, D, where it is completely dried. From D it passes through the train of weighed vessels, E, F, G and H, into a 15-litre bottle, I.

The vessels E and F contain the solution, and G and H the water and sulphuric acid respectively. The bottle, I, serves to damp down the changes of pressure caused by the air bubbling through the water in J. From J the air passes to the arrangement shown in fig. 8.

The process followed in an experiment was first of all to clean the vessels,

inside and outside, with a mixture of chromic and sulphuric acids, then after washing and drying the inside, the last branch of each vessel was filled with platinum tubes which had recently been ignited. The right quantity, about 28 c.c., of the various liquids was then measured into their respective vessels, and the latter, together with a closed counterpoise made of the same glass, and of the same shape and size as the vessels themselves, were washed and dried. It was necessary to pay great attention to the washing and drying, otherwise the amount of moisture which condenses on the vessel and counterpoise when on the balance pans will not be the same. They were, therefore, first washed with distilled water and then with pure alcohol, and finally dried with thoroughly dry fine linen dusters. The vessel and counterpoise were then weighed, against one another, by the method of double weighing,

FIG. 9.



and the temperature of the balance case and the height of the barometer noted.

A special balance, made by Oertling, was purchased for this work, because ordinary balances are not fitted with pans large enough to take the vessels, and, further, the weight of some of the earlier forms of vessel was over 250 grammes. After weighing the vessels they were secured in their places on the platform and connected together by the joints already described; the whole was then lowered into the bath and the experiment started.

The rate of passage of the air was indicated by the rate of bubbling through J (see fig. 9), and it was kept, in the later experiments, as near as may be to 50 bubbles in 28 seconds. The length of time, during which an experiment lasted, varied with different concentrations. The order of

accuracy we aimed at was 5 per cent., and to reach this it was found necessary to obtain a loss of weight, in the vessel containing the water, such that the experimental error in weighing should not exceed 5 per cent. of the total loss. The experimental error in weighing one of the vessels was found to be under $\pm$ 0·0010 gramme, hence the loss of water should be at least 0·04 gramme.

The rate of bubbling mentioned gave this loss, with the weakest solution used (285 grammes of sugar in 1000 c.c.) in about 96 hours.

When the experiment was judged to be finished the vessels were taken out of the bath and washed, dried, and weighed as before—the balance temperature and height of barometer being noted for use in the reduction of the weights to a vacuum.

The table on p. 164 is a copy from the laboratory notebook of one of the later experiments. The solution used is 660 grammes sugar in 1000 c.c.

On taking down the vessels it was noticed that some water had condensed in the tube joining G and H, and its weight could not be satisfactorily estimated.

	Weight of vessel E.	Weight of vessel F.	Weight of vessel G.	Weight of vessel H.
Weight reduced to a vacuum—				
Before the experiment	23·0951	25·5783	4·2409	81·2665
After the experiment	21·5123	25·5770	4·1091	82·9759
Difference	1·5828	0·0012	0·1318	1·7094

Losses	1·7158
Gain	1·7094
Difference	0·0064
Mean corrected temperature	19°·47 C.

For the purpose of reducing the weights to a vacuum, the capacities of the different vessels and of the counterpoise had previously been determined, and, knowing the quantity of liquid put into the tubes, the data for the reduction is at hand.

In this experiment the vessel F, containing the second solution, lost 0·0012 gramme in weight. A similar loss was always experienced with cane-sugar solutions, but it will be remembered that with two vessels containing a sodium-chloride solution no appreciable loss was observed. The only difference that we could detect in the behaviour of these two solutions, when set up in an experiment, was that, on oscillating the vessel containing the cane sugar, there is a tendency for septa of the liquid to form

Date.	Time.	Bath temp.	Weight of vessel E.	Weight of vessel F.	Weight of vessel G.	Weight of vessel H.	Temp. of balance.	Barometer.	Time taken for 50 bubbles to pass.	Remarks.
October 27 ...	9.45 A.M.		L 23·1894				10°·7	756·0		
	10.5		R 23·1939				10·8			
	10.40		23·1916	L 25·6676			10·9			
	11.0			R 25·6719						
	11.35			25·6697	L 4·3352		11·1			
	11.55				R 4·3394					
	12.30 P.M.				4·3373	L 81·3555	11·4			
	12.50					R 81·3606			secs.	
	1.10	19°·22							37	Started air current and oscillatory system.
	5.5	19·22							31	
" 28 ...	9.0 A.M.	19·22							28	
	5.15 P.M.	19·22							28	
" 29 ...	9.0 A.M.	19·23							28	
	5.45 P.M.	19·23							27	
" 30 ...	9.0 A.M.	19·24							27	Took down.
	10.30		L 21·6049				11·9	731·6		
	10.50		R 21·6094							
	11.25		21·6071	L 25·6630			12·1			
	11.45			R 25·6674						
	12.20 P.M.			25·6652	L 4·1993		12·2			
	12.40				R 4·2037					
	1.15				4·2020	L 83·0593	12·3			
	1.35					R 83·0645				
						83·0619				

across the tube just after the solution has run down to the lower end. These septa travel some distance with the air current before breaking.

With sodium chloride, however, the septa seldom form, consequently the loss of weight in the second vessel containing the sugar solution might be caused by the slight change of pressure resulting on the increase of work to be done by the air in moving the septa in the first vessel*—the loss is therefore quite analogous to that mentioned on p. 158.

The results of the experiments are given in the following tables:—

Experiments with Flat Spiral Tubes.

Date.	Concentration of cane sugar in grammes per 1000 c.c.	Temperature.	Hours run.	Rate of bubbling.	Loss in weight of solution.	Loss in weight of water.	Osmotic pressure.
1904.		°		secs.			atmos.
Nov. 12...	285	18·8	117	49	1·6219	0·0278	22·7*
" 18...	285	18·6	109	41	1·8054	0·0380	27·8
Dec. 8...	285	18·3	117	46	1·5441	0·0315	26·9
" 14...	420	18·2	69	43	0·9445	0·0317	44·0
" 19...	420	18·3	86	40	1·2882	0·0394	40·2

* This experiment was carried out before the conditions necessary for accurate weighing were realised.

Experiments with Vessels described on p. 158.

Date.	Concentration of cane sugar in grammes per 1000 c.c.	Temperature.	Hours run.	Rate of bubbling.	Loss in weight of solution.	Loss in weight of water.	Osmotic pressure.
1905.		°		secs.			atmos.
June 11...	420	19·5	47	25	1·8624	0·0671	47·3
" 14...	420	12·6	46	23	1·2136	0·0417	44·3
" 19...	420	14·2	48	24	1·0984	0·0386	45·9
Aug. 8...	420	15·7	92	37	1·3985	0·0487	46·6
Oct. 6...	660	19·0	68	28	1·5896	0·1309	105·7
" 13...	660	19·4	68	28	1·6515	0·1324	103·2
" 27...	660	19·5	68	28	1·5822	0·1318	107·0

It will be noticed that in the experiments with the spiral tubes the loss of the water is less than 0·04 gramme. These experiments were made before we had been able to appreciate the experimental errors.

* Mr. Whetham suggested this explanation to us.

The numbers under the column headed "Osmotic pressure" are calculated from the equation $P = \frac{As}{\sigma} \log_e \frac{p}{p_1}$, where P is the osmotic pressure, p the vapour pressure of the water, p_1 the vapour pressure of the solution and s the density of water at the temperature of the experiment, σ is the vapour density of water vapour under the standard atmosphere, A. The discussion of this equation is given below.

Theory.—When the results of our vapour-pressure experiments were used for calculating the osmotic pressures by means of Arrhenius' well-known relation :—

$$P = \frac{A\rho}{\sigma} \log_e \frac{p}{p_1} \tag{1}$$

(this equation differs from the one given above only in that the density of the solution ρ replaces s, the density of the solvent), it was found that they differed considerably from those measured directly.

For instance, the two values of the osmotic pressure of a solution of cane sugar in water—

Of	285	grammes to the litre	differed by	about	5	per cent.
„	420	„	„	„	15	„
„	660	„	„	„	30	„

It was thought, at first, that these discrepancies were due to some error in the vapour pressure method, and numerous experiments, previous to those given in the second table, but not tabulated, were carried out to throw light on the subject.

Eventually Equation (1) was examined more closely. As this examination led to a result which does not seem to be generally known, we take the opportunity of drawing attention to the subject. This we do the more readily, in that Mr. Spens has come independently to a similar conclusion* by starting from a different point of view.

It is necessary to recapitulate briefly part of the reasoning by which Equation (1) is derived. In fig. 10, AB is a vessel closed at the lower end by a semi-permeable membrane, B, and filled with a solution to a height BA above the solvent, C, such that the vapour pressures of the solvent and solution are in equilibrium at A.

If ρ be the density of the solution and p_1 its vapour pressure, p the vapour pressure of the solvent at C, and P the osmotic pressure; then, by considering the pressures about the membrane, it is easy to see that

$$P + p = gh\rho + p_1 \text{ (where } h = \text{BA).}$$

* See paper published *infra*.

But $p - p_1$ is very small compared to the osmotic pressure of a strong solution, and can therefore be neglected; then

$$P = ghs. \tag{2}$$

It is evident that this relation is true only when the density and concentration are the same throughout the column AB. A 30-per-cent. solution of

FIG. 10.

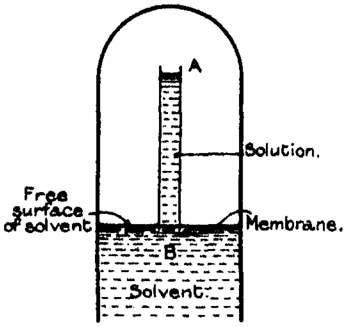
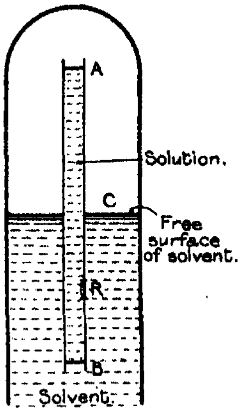


FIG. 11.



sugar in water would be in equilibrium when standing in a column some 500 yards high. In a long column such as this, there is no reason for supposing that the density and concentration are constant throughout. In fact, it is easy to show that a difference in concentration, and therefore a difference in density, must take place.

Thus in fig. 11 let the lettering have the same signification as in fig. 10; the only difference in the two figures is that the tube is prolonged downwards and the membrane B is below the surface of the solvent.

If we assume that the density of the solution is higher than that of the solvent, then the hydrostatic pressure on B is in this case greater than in fig. 10, by an amount $g\text{BC} \times \rho - g\text{BC} \times s$, where ρ and s are the densities of the solution and solvent respectively. It follows that the osmotic pressure at B, in fig. 11, is greater than that at B in fig. 10, and the only cause which can produce this is a difference of concentration at the two points.

Van Calcar and De Bruyn* have shown that solutions can be concentrated by centrifugalisation; the force of gravity acting on the solute molecules would of course produce a similar effect.

Be this as it may, if we regard it as proved that the density and concentration of the solution at the top and at the bottom of the column are

* 'Rec. Trav. Chem. Leiden,' vol. 23, pp. 218—223, 1903.

not the same, then Equation (2) will only be true if we substitute for ρ, the averaged density between the limits ρ_A and ρ_B, where ρ_A and ρ_B are the densities at the top and bottom of the column. P will then be the osmotic pressure at the bottom of the column in fig. 10.

A direct measurement of osmotic pressure evidently gives the osmotic pressure corresponding to the concentration at the top of the column, it is therefore necessary to obtain an equation connecting this with the same pressure derived by way of the vapour pressures.

The following investigation gives the required relation :—

Consider fig. 11.—If another semipermeable membrane be opened at R, the equilibrium will not be disturbed, otherwise perpetual motion would result.

If P_R denote the osmotic pressure at R, and h the height above B, then, from a consideration of the hydrostatic equilibrium of the column BR,

$$P_R + \text{pressure of column BR of solution} = P_B + \text{press. col. BR of solvent},$$

$$\therefore P_R + \int_B^R g\rho dh = P_B + \int_B^R gs dh,$$

$$\therefore P_R = P_B - \int_B^R g(\rho - s)\, dh. \tag{i}$$

But the hydrostatic equilibrium up to A gives

$$P_B = \int_B^A g\rho dh - \int_B^C gs dh - \text{pressure of column AC of vapour.} \tag{ii}$$

By a process similar to (i)

$$P_A = P_B - \int_B^A g(\rho - s)\, dh.$$

Substituting in (ii)

$$P_A = \int_B^A g\rho dh - \int_B^C gs dh - \text{vapour AC} - \int_B^A g\rho dh + \int_B^A gs dh$$

$$= \int_C^A gs dh - \text{pressure of column CA of vapour.} \tag{iii}$$

It will have been noticed that our experimental results differ from one another by about 5 per cent. The pressure of the column CA of vapour is evidently negligible compared with the osmotic pressures we are measuring; and, as compared with the experimental errors, the compressibility of the solvent, water, and the change in the force of gravity due to height above sea level, are also negligible.

Equation (iii) may therefore be replaced by $P_A = gs\text{CA}$.

The only change in Equation (2), and therefore in Equation (1), necessary to give the osmotic pressure at the top of the column is the substitu-

tion of the density of the solvent in place of the density of the solution. On making this correction it was found that the osmotic pressures derived from our vapour-pressure measurements and those observed directly, agreed to within 5 per cent.

This communication should be regarded partly in the light of a preliminary paper. It is published now, because we think the limited number of experiments are sufficient to establish the primary object we had in view, namely, to prove the method we have adopted for the direct determination of osmotic pressures. We learn from a reference in 'Science'* that Professor Kahlenberg recently read a paper before the American Chemical Society describing what appears to be a dynamic method similar to ours.

The various precautions taken have been described at some length, because we think that successful results can only be obtained by paying great attention to the details of the experiments. We are glad to have this opportunity of thanking Mr. Whetham for the kindly interest he has taken in the work.

* "On a New Dynamic Method of Measuring Vapour Tensions of Solutions," 'Science,' July 21, 1905.

Artificial Double Refraction, due to Æolotropic Distribution, with Application to Colloidal Solutions and Magnetic Fields.

By T. H. HAVELOCK, M.A., D.Sc., Fellow of St. John's College, Cambridge.

(Communicated by Professor J. Larmor, Sec. R.S. Received November 17, 1905,—Read January 25, 1906.)

In 1902 Majorana* described some new effects observed on placing in a uniform magnetic field certain solutions of dialysed iron, such as that of Bravais. The solution was found to become doubly refracting like a positive or negative uniaxal crystal, with its axis parallel to the lines of magnetic force. A formal theory of the phenomena given by Voigt† consisted in adding to the ordinary electrical equations terms of the second order, representing the action of the magnetic field upon the natural vibrations of the medium.

Previously to these experiments, Kerr‡ had obtained a negative double refraction in submitting to a magnetic field a medium composed of pure water containing invisibly fine particles of Fe_3O_4, his explanation of the effect being that the particles might be supposed to join together into filaments along the lines of magnetic force.

It appears to be in this direction that the explanation of the phenomena is to be found. For Schmauss§ noticed that the effect could only be obtained in colloidal solutions, and in recent experiments—of which preliminary accounts have been published—Cotton and Mouton|| have determined experimentally that the effect is connected with the presence of ultramicroscopic particles; it is supposed that the double refraction is due to orientation of these particles in the magnetic field.

The present investigation begins, then, with the assumption that the effects to be considered occur in colloidal solutions, consisting optically of a continuous medium, in which is imbedded a very large number of extremely minute spherical obstacles. Without at first considering the manner of producing a strain in such a medium, the packing of the obstacles is supposed to be disturbed from the cubical arrangement of the natural state.

* 'Accademia dei Lincei, Rendiconti,' vol. 11, 1, p. 531, 1902.
† 'Accademia dei Lincei, Rendiconti,' vol. 11, 1, p. 505, 1902.
‡ 'Brit. Assoc. Report,' 1901, p. 568.
§ 'Annalen der Physik.,' vol. 12, p. 186, 1903.
|| 'Comptes Rendus,' vol. 141, pp. 317 and 349, 1905.

The first section consists of a discussion of the optical properties of this medium, following Lord Rayleigh's investigation,* except that the calculations are carried out for the general case of arrangement in rectangular order, instead of for the usual case of cubical order; expressions are obtained for the indices of refraction and absorption of the medium in the three principal directions.

The second section deals with the production of the strain by means of mechanical stress, and contains a discussion of various experimental results, which show that colloidal solutions may be regarded as possessing a certain amount of rigidity. Finally, in a third section, the action of a magnetic field is considered.

The object of the paper is to point out the agreement in general character of the effects obtained by mechanical stress and by magnetic action, and their representation by the formal theory of the first section.

1. *Optical Effect of Spherical Obstacles arranged in Rectangular Order.*

It is necessary to repeat briefly some of Lord Rayleigh's analysis. The equivalent problem is considered at first of steady flow of electric current through a medium of unit specific conductivity, interrupted by spherical obstacles of conductivity ν, arranged in rectangular order. Suppose the radius of a sphere is a, and the side of the rectangle in the direction of flow is α, the two other sides being β, γ. Then it is required to find ν_a, the specific conductivity of the compound medium in the direction α. If we take the centre of one of the spheres P as origin of polar co-ordinates, the potential external to the sphere may be expanded in the series

$$V = A_0+(A_1r+B_1r^{-2})Y_1+\ldots+(A_nr^n+B_nr^{-n-1})Y_n+\ldots, \tag{1}$$

and at points internal to the sphere P in the series

$$V' = C_0+C_1Y_1r+\ldots+C_nY_nr^n+\ldots, \tag{2}$$

Y_n being the spherical surface harmonic of order n.

The conditions at the surface of the sphere are

$$V = V'; \qquad \nu\frac{\partial V'}{\partial r} = \frac{\partial V}{\partial r}. \tag{3}$$

From these, by equating coefficients of similar orders, we find

$$B_n = \frac{1-\nu}{1+\nu+1/n}a^{2n+1}A_n. \tag{4}$$

Now in general

$$Y_n = \sum_{s=0}^{s=n}\Theta_n^{(s)}(H_s\cos s\phi+K_s\sin s\phi), \tag{5}$$

* 'Scientific Papers,' vol. 4, p. 19.

where $$\Theta_n^{(s)} = \sin^s\theta \left\{ \cos^{n-s}\theta - \frac{(n-s)(n-s-1)}{2(2n-1)} \cos^{n-s-2}\theta + \dots \right\}, \tag{6}$$

θ being measured from the axis of x (parallel to α), and ϕ from the plane of xz. From considerations of symmetry, it is seen that s must be even, and that Y_n (except for n zero) should be reversed when θ is changed to $\pi-\theta$. Thus, even values of n do not occur, nor do sine terms in ϕ. Hence we have

$$Y_1 = \cos\theta; \qquad Y_3 = \cos^3\theta - \tfrac{3}{5}\cos\theta + H_2\sin^2\theta\cos\theta\cos 2\phi;$$

$$Y_5 = \cos^5\theta - \tfrac{10}{9}\cos^3\theta + \tfrac{5}{21}\cos\theta + L_2\sin^2\theta\,(\cos^3\theta - \tfrac{1}{3}\cos\theta)\cos 2\phi$$
$$+ L_4\sin^4\theta\cos\theta\cos 4\phi.$$

Also, when $\beta = \gamma$, we have in addition

$$H_2 = 0; \qquad L_2 = 0;$$

Now apply Green's theorem,

$$\iint\left(U\frac{\partial V}{\partial n} - V\frac{\partial U}{\partial n}\right) dS = 0, \tag{7}$$

to the boundary of the space between the sphere P and the surface of a rectangular parallelopiped, with edges, α, β, γ placed symmetrically between the spheres; and take

$$U = x = r\cos\theta.$$

The four faces parallel to the planes $\alpha\beta$ and $\gamma\alpha$ are, from symmetry, made up of lines of flow of the current; hence, on these, both $\partial U/\partial n$ and $\partial V/\partial n$ are zero.

Further, let C be the total current across the face $\beta\gamma$, and V_1 the fall in potential along the edge α; then the contribution of the two remaining faces to the integral in (γ) is $\alpha C - \beta\gamma V_1$.

Also, on the sphere, we have

$$U = a\cos\theta; \qquad \partial U/\partial n = -\cos\theta;$$

thus the integral over the surface of the sphere gives $4\pi B_1$. Hence we have

$$\alpha C - \beta\gamma V_1 + 4\pi B_1 = 0. \tag{8}$$

The potential V at any point may be regarded as due to external sources at infinity (by which the flow is caused), and to multiple sources situated at the centres of the spheres; we may denote the first part by Hx. Further, for simplicity in taking account of the multiple sources, we must suppose that the system of obstacles, though of infinite extent in every direction, is yet infinitely more extended in the direction α than in the directions β, γ. Then the periodic difference of potential V_1 may be considered as entirely due to Hx, and put equal to $H\alpha$.

Then from (8) we have

$$\frac{C}{V_1} = \frac{\beta\gamma}{\alpha}\left(1 - \frac{4\pi B_1}{\alpha\beta\gamma H}\right), \tag{9}$$

and the required specific conductivity is given by

$$\nu_a = 1 - 4\pi B_1/\alpha\beta\gamma H. \tag{10}$$

Now consider the potential at a point (x, y, z) near the sphere P, whose centre is the origin of co-ordinates. The potential due to Hx and to the other spheres Q regarded as multiple sources is given by part of expression (1), viz.:—

$$A_0 + A_1 r Y_1 + A_3 r^3 Y_3 + \dots;$$

but if (x', y', z') be the same point referred to one of the spheres Q, the potential due to these spheres must be given also by a summation:

$$B_1 \Sigma \frac{Y_1'}{r'^3} + B_3 \Sigma \frac{Y_3'}{r'^7} + \dots .$$

Hence we have

$$A_0 + (A_1 - H)x + A_3(x^3 - \tfrac{3}{5}xr^2) + \dots = B_1 \Sigma \frac{x'}{r'^3} + B_3 \Sigma \frac{x'^3 - \frac{3}{5}x'r'^2}{r'^7} + \dots . \tag{11}$$

Let (ξ, η, ζ) be the coordinates of Q referred to P, so that

$$(x', y', z') = (x - \xi, y - \eta, z - \zeta).$$

The left-hand side of (11) is the expansion of the right in ascending powers of x, y, z. Thus $A_1 - H$ is formed by taking d/dx of the right-hand side and making x, y, z zero, and so on.

Now, at the origin,

$$\frac{d}{dx}\frac{x'}{r'^3} = -\frac{d}{d\xi}\frac{x'}{r'^3} = -\frac{d}{d\xi}\frac{-\xi}{\rho^3} = \frac{\rho^2 - 3\xi^2}{\rho^5} = -2\rho^{-3}P_2(\mu),$$

where

$$\rho^2 = \xi^2 + \eta^2 + \zeta^2, \qquad \mu = \xi/\rho.$$

Similarly,

$$\frac{d}{dx}\frac{x'^3 - \frac{3}{5}x'r'^2}{r'^7} = \frac{d}{d\xi}\frac{\xi^3 - \frac{3}{5}\xi\rho^2}{\rho^7} = -\tfrac{8}{5}\rho^{-5}P_4(\mu);$$

$$\frac{d^3}{dx^3}\frac{x'}{r'^3} = -24\rho^{-5}P_4(\mu).$$

Thus from comparison of terms in (11) we have

$$\left.\begin{aligned} A_1 - H &= -2B_1\Sigma\rho^{-3}P_2 - \tfrac{8}{5}B_3\Sigma\rho^{-5}P_4 + \dots \\ A_3 &= -4B_1\Sigma\rho^{-5}P_4 + \dots \\ &\dots\dots\dots\dots\dots\dots \end{aligned}\right\} \tag{12}$$

In each of the quantities, such as $\Sigma\rho^{-3}P_2$, the summation extends to all the points whose co-ordinates are of the form $(l\alpha, m\beta, n\gamma)$, where l, m, n are any set of integers, positive or negative, except $(0, 0, 0)$.

From (12) and (4) we get

$$\frac{\mathrm{H}a^3}{\mathrm{B}_1} = \frac{2-\nu}{1-\nu} + 2a^3\Sigma\rho^{-3}\mathrm{P}_2 - \tfrac{32}{5}\frac{1-\nu}{\frac{4}{3}+\nu}a^{10}(\Sigma\rho^{-5}\mathrm{P}_4)^2 + \dots . \tag{13}$$

Supplying this value in (10) the value of the conductivity ν_a can be calcalated. We shall carry the approximation only as far as to calculate the value of the quantity $\Sigma\rho^{-3}\mathrm{P}_2$. We have

$$\mathrm{S}_2 = \Sigma\rho^{-3}\mathrm{P}_2 = \Sigma\frac{3\mu^2-1}{2\rho^3} = \Sigma\frac{2\xi^2-\eta^2-\zeta^2}{2\rho^5} = -\tfrac{1}{2}\Sigma\frac{\partial}{\partial\xi}\left(\frac{\xi}{\rho^3}\right). \tag{14}$$

Now we have considered the system of obstacles to be infinitely more extended *in the direction* α *than in the directions* β *and* γ; *thus* we must regard the summation S_2 as made up of two parts, Σ_1 and $2\Sigma_2$. In Σ_1 the summation extends over the region between the sets of planes given by

(i) $\xi = \pm\alpha v_1$; $\eta = \pm\beta v_1$; $\zeta = \pm\gamma v_1$;

(ii) $\xi = \pm\alpha v$; $\eta = \pm\beta v$; $\zeta = \pm\gamma v$;

where ultimately v_1 is made small so as to exclude the origin only, and v may be increased indefinitely.

In Σ_2 the summation extends over the region bounded by the planes

$$\xi = \alpha v; \quad \xi = \infty; \quad \eta = \pm\beta v; \quad \zeta = \pm\gamma v.$$

To evaluate Σ_1 and Σ_2 we shall suppose that α, β, γ are very small, the number of obstacles in any small space being very large. Then the summation may be replaced by integration, and putting $(\xi, \eta, \zeta) = (l\alpha, m\beta, n\gamma)$ we have from (14)

$$\mathrm{S}_2 = -\tfrac{1}{2}\iiint\frac{\partial}{\partial l}\frac{l}{(\alpha^2l^2+\beta^2m^2+\gamma^2n^2)^{\frac{3}{2}}}\,dl\,dm\,dn,$$

where the limits of the integration are to be arranged in the manner specified. On working this out, it is easily found that Σ_1 is zero, and

$$\begin{aligned} 2\Sigma_2 &= -\int_{-v}^{+v}\int_{-v}^{+v}\int_{+v}^{\infty}\frac{\partial}{\partial l}\frac{l}{(\alpha^2l^2+\beta^2m^2+\gamma^2n^2)^{\frac{3}{2}}}\,dl\,dm\,dn \\ &= \frac{4}{\alpha\beta\gamma}\sin^{-1}\frac{\beta\gamma}{\sqrt{(\alpha^2+\beta^2)(\alpha^2+\gamma^2)}}. \end{aligned} \tag{15}$$

Hence from (13) and (10) we find

$$1-\nu_a = \frac{4\pi}{\alpha\beta\gamma}\bigg/\left(\frac{2+\nu}{1-\nu}\,\frac{1}{a^3} + \frac{8}{\alpha\beta\gamma}\sin^{-1}\frac{\beta\gamma}{\sqrt{(\alpha^2+\beta^2)(\alpha^2+\gamma^2)}}\right).$$

If we write

$$p = \frac{4\pi a^3}{3\alpha\beta\gamma} = \text{proportional volume occupied by the spheres},$$

$$\delta_\alpha = \frac{2}{\pi}\sin^{-1}\frac{\beta\gamma}{\sqrt{(\alpha^2+\beta^2)(\alpha^2-\gamma^2)}},$$

we have

$$\frac{\nu_\alpha - 1}{\nu_\alpha + \delta_\alpha^{-1} - 1}\frac{1}{p} = \frac{3\delta_\alpha(\nu-1}{\nu+2}. \tag{16}$$

In applying this formula to wave propagation, it is necessary to assume the wave-length to be large compared with the lengths α, β, γ specifying the structure of the medium. Then if

μ' = refractive index for the spheres,
μ = „ „ for the surrounding medium,
μ_α = „ „ for the compound medium for vibrations parallel to the direction α,

we have

$$\frac{\mu_\alpha^2 - \mu^2}{\mu_\alpha^2 + (\delta_\alpha^{-1}-1)\mu^2}\frac{1}{p} = \frac{3\delta_\alpha(\mu'^2-\mu^2)}{\mu'^2+2\mu^2}. \tag{17}$$

The indices μ_α, μ_β, μ_γ, are in general different from each other; but if α, β, γ are all equal, so that the obstacles are in cubical order, we have

$$\delta_\alpha = \delta_\beta = \delta_\gamma = \frac{2}{\pi}\sin^{-1}\tfrac{1}{2} = \tfrac{1}{3},$$

and in this case (17) reduces to the usual form,

$$\frac{\mu_0^2-\mu^2}{\mu_0^2-2\mu^2}\frac{1}{p} = \frac{\mu'^2-\mu^2}{\mu'^2+2\mu^2}. \tag{18}$$

Equation (17) may be compared with an empirical formula, which has been used in expressing the relation between refractive index and density, viz.:—

$$(\mu^2-1)/(\mu^2+c)p = \text{constant}.$$

We shall consider now the case in which

$$\beta = \gamma = \alpha(1+\epsilon), \text{ where } \epsilon \text{ is small}.$$

The medium is supposed to be such that while there are many small spheres to the wave-length, yet the proportional volume p is small. We may also, for further illustration, suppose the spheres to possess some conducting power; to express this we replace μ' by the complex quantity $\mu'(1-i\kappa')$ Then (17) becomes

$$\frac{\mu_\alpha^2(1-i\kappa_\alpha)^2-\mu^2}{\mu_\alpha^2(1-i\kappa_\alpha)^2+(\delta_\alpha^{-1}-1)\mu^2}\frac{1}{p} = \frac{3\delta_\alpha\{\mu'^2(1-i\kappa')^2-\mu^2\}}{\mu'^2(1-i\kappa')^2+2\mu^2}. \tag{19}$$

To the second order in p this gives

$$\frac{\mu_\alpha^2(1-i\kappa_\alpha)^2}{\mu^2} = 1+3\,\frac{\mu'^2(1-i\kappa')^2-\mu^2}{\mu'^2(1-i\kappa')^2+2\mu^2}\,p+9\left\{\frac{\mu'^2(1-i\kappa')^2-\mu^2}{\mu'^2(1-i\kappa')+2\mu^2}\right\}^2\delta_\alpha p^2. \quad (20)$$

And for μ_β, the index for vibrations parallel to the direction β, we have a similar expression with

$$\delta_\beta = \frac{2}{\pi}\sin^{-1}\frac{\gamma\alpha}{\sqrt{(\beta^2+\gamma^2)(\beta^2+\alpha^2)}}.$$

If we put

$$\frac{\mu'^2(1-i\kappa')^2-\mu^2}{\mu'^2(1-i\kappa')^2+2\mu^2} = \frac{\mu'^4(1+\kappa'^2)^2+\mu^2\mu'^2(1-\kappa'^2)-2\mu^4-6i\mu^2\mu'^2\kappa'}{\mu'^4(1+\kappa'^2)^2+4\mu^2\mu'^2(1-\kappa'^2)+4\mu^4} = r-is,$$

then

$$\mu_\alpha^2(1-\kappa_\alpha^2)/\mu^2 = 1+3rp+9\delta_\alpha(r^2-s^2)p^2,$$

$$2\mu_\alpha^2\kappa_\alpha/\mu^2 = 3sp+18\delta_\alpha rsp^2.$$

These give, to the second order in p, the values

$$\mu_\alpha^2/\mu^2 = 1+3rp+9\,\{\tfrac{1}{4}s^2\mu^4+\delta_\alpha(r^2-s^2)\}\,p^2, \quad (21)$$

$$2\kappa_\alpha/\mu^2 = 3sp+9\,(2\delta_\alpha-1)\,rsp^2. \quad (22)$$

Similarly, for vibrations parallel to β,

$$\mu_\beta^2/\mu^2 = 1+3rp+9\,\{\tfrac{1}{4}s^2\mu^4+\delta_\beta(r^2-s^2)\}\,p^2, \quad (23)$$

$$2\kappa_\beta/\mu^2 = 3sp+9\,(2\delta_\beta-1)\,rsp^2. \quad (24)$$

Further, if quantities with suffix zero correspond to a similar medium with the same concentration p, but with the spheres arranged in cubical order, we have

$$\mu_0^2/\mu^2 = 1+3rp+9\,\{\tfrac{1}{4}s^2\mu^4+\delta_0(r^2-s^2)\}\,p^2, \quad (25)$$

$$2\kappa_0/\mu^2 = 3sp+9\,(2\delta_0-1)rsp^2. \quad (26)$$

Then from these expressions we obtain the following results:—

$$(\mu_\alpha^2-\mu_\beta^2)/\mu^2 = 9\,(r^2-s^2)\,(\delta_\alpha-\delta_\beta)\,p^2,$$

or, to the same approximation,

$$2\,(\mu_\alpha-\mu_\beta)/\mu = 9\,(r^2-s^2)\,(\delta_\alpha-\delta_\beta)\,p^2. \quad (27)$$

And, since $\beta=\gamma$, we have also

$$\mu_\beta = \mu_\gamma; \qquad \kappa_\beta = \kappa_\gamma.$$

Hence the medium behaves like a uniaxal crystal with the direction α for its optic axis. The index of refraction for the extraordinary ray is μ_α, while μ_β is the ordinary index; the double refraction is proportional to $\delta_\alpha-\delta_\beta$, and the crystal is positive or negative according as δ_α is greater or less than δ_β.

Now we have supposed the alteration in the structure to be small; thus we have

$$\frac{\pi}{2}(\delta_\alpha-\delta_\beta) = \sin^{-1}\frac{\beta\gamma}{\sqrt{(\alpha^2+\beta^2)(\alpha^2+\gamma^2)}}-\sin^{-1}\frac{\gamma\alpha}{\sqrt{(\beta^2+\gamma^2)(\beta^2+\alpha^2)}}$$

$$= \sin^{-1}\tfrac{1}{2}(1+\epsilon)-\sin^{-1}\tfrac{1}{2}(1-\tfrac{1}{2}\epsilon) = \tfrac{1}{2}\sqrt{3}\epsilon. \qquad (28)$$

Thus the double refraction has the value

$$\mu_\alpha-\mu_\beta = \frac{9\sqrt{3}}{2\pi}(r^2-s^2)\,\mu\epsilon p^2. \qquad (29)$$

The birefringence changes sign with ϵ, and r^2-s^2 is positive; hence if the original medium μ_0 in cubical order be relatively contracted along the direction α it becomes a positive uniaxal crystal, while if it be relatively extended it becomes a negative crystal.

Further, we have

$$\tfrac{1}{2}\pi(\delta_\alpha-\delta_0) = \sin^{-1}\tfrac{1}{2}(1+\epsilon)-\sin^{-1}\tfrac{1}{2} = \sin^{-1}\epsilon/\sqrt{3} = \epsilon/\sqrt{3},$$

$$\tfrac{1}{2}\pi(\delta_0-\delta_\beta) = \sin^{-1}\tfrac{1}{2}-\sin^{-1}\tfrac{1}{2}(1-\tfrac{1}{2}\epsilon) = \tfrac{1}{2}\epsilon/\sqrt{3}.$$

Hence, from (25) we have

$$\left.\begin{aligned}\mu_\alpha-\mu_0 &= \frac{9}{\pi\sqrt{3}}(r^2-s^2)\,\mu\epsilon p^2\\ \mu_0-\mu_\beta &= \frac{9}{2\pi\sqrt{3}}(r^2-s^2)\,\mu\epsilon p^2\end{aligned}\right\} \qquad (30)$$

Thus if the double refraction is negative, the diminution in index for vibrations parallel to the axis is double the increase in index for vibrations perpendicular to the axis; and conversely if the medium becomes a positive crystal, the extraordinary index in both cases undergoing the greater change. Also the medium becomes selectively absorbing, since we find

$$\kappa_\alpha-\kappa_\beta = \frac{9\sqrt{3}}{\pi}\,rs\mu^2\epsilon p^2. \qquad (31)$$

Thus if the double refraction is positive, vibrations parallel to the axis are more absorbed than those at right angles to the axis; and conversely if the medium is negative. In either case the more slowly moving waves undergo the greater absorption, and this unequal absorption gives rise to an apparent rotation of the plane of polarisation.

2. *Double Refraction Produced by Mechanical Stress.*

In 1866 Maxwell* obtained double refraction in a jelly of isinglass by means of mechanical stress. The substance was poured, when hot, between

* 'Scientific Papers,' vol. 1, p. 43.

two coaxial cylinders; one of these was twisted, and the jelly viewed by polarised light paralled to the axis of the cylinders. There was found to be a difference of retardation of oppositely polarised rays due to the shearing stress at any point in the jelly, and further the planes of polarisation of the rays were inclined at 45° to the radius to the point. In addition, if the force of torsion was continued while the jelly cooled and was then removed, the double refraction still persisted.

Maxwell also noticed later a similar effect with Canada balsam, and the subject has been studied further by Kundt,* De Metz,† Umlauf,‡ and Almy;§ the point of importance in these experiments is that quite dilute solutions were used, and that the effect was clearly observed with colloids in solution, but could not be detected with crystalloids.

In 1889 Schwedoff‖ observed directly, by a torsion balance, the existence of rigidity in certain liquids, and in the case of a dilute solution of gelatine found for the modulus of rigidity the value 0·535 dyne per square centimetre; he also remarks that a liquid may be very fluid and yet have a measurable rigidity.

De Metz,¶ in studying the double refraction of liquids under stress, showed that the order of magnitude of the effect in different liquids is not the same as the order of their viscosity. In a later paper he measures the time taken for the disappearance of the double refraction after removal of the stress, and, with some assumptions, calculates therefrom the modulus of rigidity; for copal varnish he arrives at the value 0·12 dyne per square centimetre, which is of the same order of magnitude as Schwedoff's result.

Some experiments by Hill** on accidental double refraction in liquids may be considered in more detail.

The liquid was contained in a cast-iron chest, within which were two parallel cylinders, external to one another and capable of rotation. The liquid was viewed by polarised light passing parallel to the cylinders in the space between them, and the double refraction due to the shearing stress produced by rotating the cylinders was measured by a half-shade polariscope which allowed very minute effects to be detected. The following results were obtained with dilute solutions of gelatine.

For small velocities of the cylinders the double refraction increases with

* 'Wied. Ann.,' vol. 13, p. 110, 1881.
† 'Wied. Ann., vol. 35, p. 497, 1888.
‡ 'Wied. Ann.,' vol. 45, p. 304, 1892.
§ 'Phil. Mag.,' vol. 44, p. 499, 1897.
‖ 'Paris Congress Reports,' 1900, vol. 1, p. 478.
¶ 'Comptes Rendus,' vol. 134, p. 1353, 1902; also vol. 136, p. 604, 1903.
** 'Phil. Mag.,' vol. 48, 2, p. 485, 1899; also vol. 2, 2, p. 524, 1901.

the speed of rotation, though not in proportion to it. This increase continues up to a certain point, when an elastic limit seems to be reached; beyond this point the amount of double refraction decreases and finally changes sign as the velocity is increased. This result of a maximum effect, with subsequent breakdown, was also observed with static strains; even with a very dilute jelly, almost as fluid as water, double refraction was obtained by stress. With water and solutions of crystalloids no effect could be detected. The amount of double refraction appeared to be proportional to the concentration, and also varied much with the previous history of any given solution.

The results may be classified, for comparison, under three types:—

1. Very dilute solution giving weak positive effect without reversal.

2. Solution with positive effect passing through a maximum and a point of reversal to a negative effect which increases with the stress.

3. Solution with same concentration as 2, but, after being heated and cooled, giving larger positive effect with point of inversion at higher speed.

Using more concentrated gelatine solutions, Leick* has found that the double refraction is proportional to the product of the relative elongation and the concentration, while the modulus of elasticity varies approximately as the square of the concentration.

In comparing these various results with the strain theory of the previous section, we notice also a remark made by Maurer,† and long ago by Stokes, viz., that on straining slightly a gelatine solution, the transverse contraction is one-half the longitudinal extension: for the volume dilatation is clearly negligible.

Hence it appears that we may regard a colloidal solution as possessing rigidity, or resistance to shearing strain. Consider the application of a pressure, P, to two opposite faces of unit cube of such a substance; then, if n is the modulus of rigidity, we have the longitudinal contraction equal to $P/3n$, and the lateral extension equal to $P/6n$.

In (29) we found the double refraction to be proportional to ϵp^2, and in this case we should have ϵ equal to $P/2n$. Thus, if the modulus of rigidity is proportional to the concentration, the double refraction also varies as the concentration, that is, within the elastic limits. Experiment shows that after a certain amount of shearing the structure breaks down and a negative effect sets in.

* 'Ann. der Physik,' vol. 14, p. 139, 1904.
† 'Wied. Ann., vol. 28, p. 628, 1886.

3. *Double Refraction Due to a Magnetic Field.*

Before comparing the results in this case with those for mechanical stress, we may notice a confirmation of some formulæ of the first section in an observation made by Cotton and Mouton, who were able to obtain liquids active enough to give the following result:—

A hollow prism filled with the liquid is placed between the poles of an electromagnet and a ray of monochromatic light passes through the prism. When the field is put on, the ray is doubled into two components polarised at right angles to each other. Further, the two components are placed on opposite sides of the primitive ray, but unsymmetrically; the vibrations parallel to the lines of force undergo a diminution of index about double the increase of the index for perpendicular vibrations. This holds for a negative double refraction; with a positive effect the positions of the two components are reversed, but the vibrations parallel to the field are still the more displaced from the original ray.

But this is precisely the result obtained in (30), viz.:—*

$$\mu_a - \mu_0 = \frac{9}{\pi\sqrt{3}}(r^2 - s^2)\,\mu\epsilon p^2,$$

$$\mu_0 - \mu_\beta = \frac{9}{2\pi\sqrt{3}}(r^2 - s^2)\,\mu\epsilon p^2,$$

where ϵ is positive for a positive double refraction.

For the variation of the effect with the magnetic field strength, solutions may be classed under various types, due originally to Majorana and extended by Cotton and Mouton. We have then the following types:—

1. A very weak positive effect, increasing slightly with the field strength H.

2. A similar negative effect.

3. A positive effect, with moderate values of H, increasing to a maximum, then decreasing to a point of inversion and becoming negative: for very large values of H, the large negative effect varying approximately as H^2.

4. A solution giving a large positive effect, increasing almost as H^2 and without inversion.

5. A solution giving weak positive effect, which increases rapidly at first

* [*December* 7.—Professor Larmor points out to me that more recently Cotton and Mouton have communicated a theoretical explanation of this result to the Société Française de Physique, which has not yet been published. He also points out that the same result follows, in the more usual procedure (*cf.* Larmor, 'Phil. Trans.,' A, vol. 190, 1897, p. 232), by taking the local interaction on a molecule to arise from a distribution on an equivalent cavity containing the molecule, which is now slightly ellipsoidal instead of spherical.]

with H, and then becoming almost constant, but increasing slightly with the field.

6. A similar negative effect.

Consider now the stress due to a magnetic field H in a medium of magnetic susceptibility κ. It is known that the mechanical stress in the medium is composed of two parts, one proportional to κH^2 and another proportional to $\kappa^2 H^2$;* and since κ is small in the fluid media under consideration, the second term may be neglected. Also κ can be found for the compound medium by the same analysis as was used for finding the refractive index; in fact, if the magnetic permeability of the simple medium be unity and that of a spherical obstacle be n, we have to the first order in p

$$4\pi\kappa = 3\frac{n-1}{n+2}p.$$

Thus κ may be positive or negative according as the spherical particles are para- or diamagnetic with respect to the surrounding medium.

Then from (29) we have for the double refraction,

$$\mu_\alpha - \mu_\beta \propto \frac{\kappa p^2}{n} H^2. \tag{32}$$

Now κ is proportional to p, and if, as in Leick's experiments, the rigidity n varies as p^2, we have the double refraction varying as the concentration.

For the variation of the double refraction with the sign of κ, we have the solutions given in types 5 and 6; type 5, with a positive effect, was an iron colloidal solution prepared by Bredig's method, while type 6 was a solution containing calcium carbonate.

We have also the following considerations from (32). If, by reason of strong concentration or large field strength, the conditions are such that the rigidity is constant for variations of H in the region considered, then the double refraction varies as the square of H; this applies to type 3 for the negative effect with large values of H, and to type 4 for the large positive effect obtained after continued heating of the solution. In the region of inversion, when such occurs, n varies with the field as in the analogous phenomenon with mechanical stress. The physical cause of such an inversion is obscure; but it is conceivable, with the particular kind of medium, that when the structure has broken down after a certain deformation, further externally-applied pressure in reality relieves some internal stress.

In the two cases of mechanical stress and magnetic action a further connection is obtained if one studies the persistence of the effects after the exciting cause has been removed.

* *Cf.* Larmor, 'Roy. Soc. Proc.,' vol. 52, p. 63, 1892.

In conclusion, the sections of the present paper may be summarised as follows:—

1. The formal investigation of artificial double refraction in colloidal solutions as due to a deformation of the medium consisting of a change in the packing of the colloidal particles.

2. The possibility that such deformation may be produced by mechanical stress as arising from the possession of a certain amount of rigidity by such solutions.

3. The analogy between the effects so produced and the double refraction due to a magnetic field.

On the Simple Group of Order 25920.

By W. Burnside, F.R.S.

(Received December 15, 1905,—Read January 25, 1906.)

To discuss the properties of a group of finite order some concrete form of representation of the group is necessary, except perhaps in the simplest cases. What are called the abstract defining relations (viz., a system of relations of the form

$$A^i = 1, \quad B^j = 1, \quad \ldots\ldots$$
$$A^pB^q\ldots = 1, \quad A^{p'}B^{q'}\ldots = 1, \quad \ldots\ldots$$

between a system of non-commutative symbols A, B, ..., which are necessary and sufficient to ensure that only a finite number of distinct products can be formed from them) no doubt contain implicitly in the most concise form all the properties of the group. To establish the properties, however, on this basis is not in general practicable. For every group there are an infinite variety of possible concrete representations; and in general for an adequate discussion of the properties of the group several of them have to be made use of. In a limited class of cases, including, however, several groups of great importance in analysis, a representation as a group of space-collineations is available. In all such cases it would be expected that this form of representation, as affording scope for space-intuition, would certainly be one of those chosen for discussion. Except, however, as regards the so-called groups of the regular solids, *i.e.*, groups of rotations round a point, this has not been done.

It is proposed in this memoir to discuss the simple group of order 25920

entirely from the point of view of projective geometry. The existence of a group of collineations of this order is not assumed, but is shown to follow from the existence of a remarkable configuration of points, lines and planes in space. This configuration itself arises naturally in connection with a much less complex group of space-collineations. The method followed throughout is synthetical and constructive. To avoid unduly burdening the earlier part of the memoir, it is assumed that the projective groups of finite order on the straight line have been established (as they can be) without appeal to analysis. Further, the simpler properties of the permutation-groups of 4, 5 and 6 symbols are taken as known.

The simple group of order 25920 has formed the subject of very many investigations; but with a single exception the present memoir is not directly connected with any of them, except of course as regards subject-matter and the better known results. The single exception is the inaugural dissertation of Herr A. Witting.* In his work Herr Witting starts from the simple group of order 25920, defined as a group of collineations by the analytical form of its five generating substitutions, and deduces from these premises the existence of the above-mentioned configuration of points, lines and planes. As stated above, the course followed here is to establish independently the existence of the configuration by considerations of projective geometry, and to deduce the existence and properties of the group.

The Abelian group, of order 16, of collineations of order 2, and the group of order 16·720 that contains it self-conjugately, with a discussion of which this investigation commences, are familiar in their analytical form to geometers in connection with a family of Kummer surfaces that possess 16 assigned nodes. In particular the existence of the larger group of order 16·720 has been made immediately obvious in a memoir of Professor Klein† by using a suitably chosen system of line-co-ordinates. For the purposes of this memoir it has, however, been necessary to establish the existence of these groups, and the nature of the configuration of 15 line-pairs which is invariant for them, directly from purely geometrical considerations, or the later developments would have been unintelligible.

* A Witting, 'Über eine Configuration in Raume auf welche die Transformationstheorie der Hyperelliptischen Functionen ($p = 2$) führt.' Gottingen, 1887. I had no knowledge of Herr Witting's work till after the whole of the present memoir had been written.

† F. Klein, 'Mathematische Annalen,' vol. 2, p. 198.

I. *On the Abelian* G_{16}.

1. A projective transformation of order 2 of a straight line has two distinct fixed points (real or imaginary). If these are A and A′, any second projective transformation of order 2 of the line which is permutable with the previous one must permute A and A′. The most general group of projective transformations of the line whose operations are all of order 2, and all permutable with each other, must be a G_4. This contains, besides identity, three operations of order 2, such that each permutes the two fixed points of the other two. If A, A′; B, B′; C, C′, are the three pairs of fixed points, each range ABA′B′, ACA′C′, BCB′C′ is harmonic; and apart from projective transformations, the configuration of the three pairs of points is absolutely determinate. Not more than two of the pairs can be real; but the distinction between real and imaginary points is, for the present purpose, unessential.

A projective transformation of order 2 of space must leave either every point of a plane ABC and some one point O not in the plane fixed; or it must leave every point of two non-intersecting lines AB and CD fixed. In each case the transformation is completely determined by its fixed points. In the first case, if P is any point, and if OP meets the plane ABC in o, then P is changed into p, lying on O Po, so that OPop is harmonic. In the second case, if a line through P meets AB and CD in Q and q, then P is changed into p, lying on QPq, so that QPqp is harmonic.

In the first case any projective transformation which leaves O and the plane ABC unchanged is permutable with the given transformation of order 2. In the second case any projective transformation which either leaves unchanged or permutes AB and CD is permutable with the given transformation.

If two transformations of the first kind (or perspectives) are permutable, the fixed point of each must be in the fixed plane of the other; so that A and B may be taken as the fixed points, and BCD, CDA as the fixed planes. If a third perspective is permutable with each of these, its fixed point must be in CD, and its fixed plane must pass through AB. Hence C may be taken as its fixed point, and ABD as its fixed plane. Then the perspective of which D is the fixed point, and ABC the fixed plane (which is the product of the three former ones), is permutable with each of them; and it is obvious that no other perspective can be permutable with each of the four. The products of these perspectives two and two are the three projective transformations of order 2 for which AB, CD; AC, BD; AD, BC are the

pairs of fixed lines, and these, with the four perspectives and identity, constitute an Abelian group of order 8, whose operations are of order 2.

Now in any Abelian group of collineations of order 2 the number of perspectives, if any, must be half the order of the group; and therefore, if the order of the group is greater than 8, it can contain no perspectives.

2. I proceed then to consider the Abelian groups of collineations of order 2 which contain no perspectives. If L_1 and L_2 are the fixed lines of one of the collineations of such a group, every collineation of the group must either permute L_1 and L_2, or must leave each of them unchanged. Moreover, the fixed lines of a collineation which leaves both L_1 and L_2 unchanged must meet both L_1 and L_2; and the fixed lines of a collineation which permutes L_1 and L_2 meet neither of them.

Further, the only collineation, other than identity, of the group which leaves every point on L_1 unchanged, is that of which L_1 and L_2 are the fixed lines; for a collineation of order 2 which leaves every point on one line and just two points on some non-intersecting line unchanged is necessarily a perspective.

Now it has been seen that the greatest Abelian group of projective transformations of order 2 of a straight line is a G_4; hence the greatest Abelian group of collineations of order 2 which changes each of two given non-intersecting lines into itself has order not exceeding 8; and the greatest such group which either permutes or changes into themselves two given non-intersecting lines has order not exceeding 16.

That such a G_{16} actually exists may be shown by the following construction. Let L_1, L_2 be any two non-intersecting lines, and on L_1 take three pairs of points A, A′; B, B′; C, C′, so that each range ABA′B′, BCB′C′, CAC′A′ is harmonic. Let M_1 and M_2 be the fixed lines of a collineation of order 2 which permutes L_1 and L_2, and denote by a, a'; b, b'; c, c' the points on L_2 into which this collineation changes A, A′; B, B′; C, C′.

Any collineation of order 2 (not a perspective) is completely specified by its two fixed lines; so that $[L_1, L_2]$ may be used to denote the collineation of order 2 which leaves every point of L_1 and of L_2 unchanged.

With this notation $[L_1, L_2]$ is clearly permutable with

$$[Aa, A'a'];\quad [Aa', A'a];\quad [Bb, B'b'];\quad [Bb', B'b];\quad [Cc, C'c'];\quad [Cc', Cc],$$

since the fixed lines of the six last written collineations meet both L_1 and L_2. Moreover, these six collineations are permutable among themselves; in fact, $[Aa, A'a']$ leaves the lines Aa' and $A'a$ unchanged and permutes the pairs Bb and $B'b'$, Bb' and $B'b$, Cc and $C'c'$, Cc' and $C'c$. Hence the above seven collineations with identity form a set of eight permutable collineations each of

which leaves both L_1 and L_2 unchanged. But it is shown above that the order of an Abelian group of collineations of order 2, which leaves L_1 and L_2 unchanged, cannot exceed 8. Hence,

$$1;\ [L_1, L_2];\ [Aa, A'a'];\ [Aa', A'a];\ [Bb, B'b'];\ [Bb', B'b];\ [Cc, C'c'];\ [C'c, Cc']$$

constitute an Abelian G_8 whose operations are all of order 2, for which each of the lines L_1 and L_2 is unchanged. This G_8 does not contain $[M_1, M_2]$ which permutes L_1 and L_2; Aa' and $A'a$; Bb' and $B'b$; Cc' and $C'c$, and leaves unchanged Aa, $A'a'$, Bb, $B'b'$, Cc, $C'c'$. Hence $[M_1, M_2]$ is not contained in the G_8, and is permutable with each of its operations. Therefore G_8 and $[M_1, M_2]$ generate an Abelian G_{16}, whose operations are all of order 2 and either permute or leave unchanged L_1 and L_2. The existence of the group is thus proved.

Returning now to the construction, the essential elements of it are the lines L_1, L_2, M_1 and the points A, A′, B; in fact, from A, A′, B the other three points B′, C, C′ on L_1 are constructed by harmonic section. If from these six points lines are drawn to meet L_2 and M_1, the points in which they meet L_2 are a, a', b, b', c, c'. Further, if Aa, $A'a'$ meet M_1 in P and P′, and if $APap$, $A'P'a'p'$ are harmonic, then pp' is M_2.

Now by a suitable projective transformation any three non-intersecting lines may be changed into L_1, L_2, M; and by a subsequent transformation which leaves these three lines unchanged, any three points on L_1 may be changed into A, A′ and B. Hence any two Abelian G_{16}'s whose operations are all collineations of order 2 are conjugate within the general group of collineations.

II. *The Invariant Configuration of the* G_{16}.

3. I consider now the configuration formed by the 15 pairs of fixed lines of the 15 collineations, other than identity, contained in such a G_{16}. Each line of any pair is unaltered by a G_8, and the two lines are permuted by the other eight collineations making up the G_{16}. Hence each line of a pair is met by both the lines of six other pairs, and is not met by either of the lines of the remaining eight pairs. Consider two pairs, say L_1 and L_2, M_1 and M_2, which do not intersect. Such a pair of pairs can be chosen in 60 ways. Both L_1 and L_2 are unchanged for the collineations of a G_8; and both M_1 and M_2 are unchanged by the operations of another G'_8. These two sub-groups of the G_{16} have a G_4 in common. If R_1, R_2; S_1, S_2; T_1, T_2 are the fixed lines of the collineations, other than identity, in this G_4, each of them must meet L_1, L_2, M_1 and M_2. Since $[L_1, L_2]$ and $[M_1, M_2]$ both leave R_1, R_2, S_1, S_2, T_1, T_2 unchanged, so also does the collineation which is the product of $[L_1, L_2]$ and

$[M_1, M_2]$. Hence if N_1 and N_2 are the fixed lines of this collineation, the six pairs

$$L_1, L_2;\ M_1, M_2;\ N_1, N_2;$$

and

$$R_1, R_2;\ S_1,\ S_2;\ T_1,\ T_2;$$

lie on a quadric, the first three belonging to one system of generators and the last three to the other. If R_1, R_2 meet L_1 in A and A′ and L_2 in a and a', then, as seen above, among the collineations of the G_{16} which leaves both L_1 and L_2 unchanged, is that for which Aa' and $A'a$ are the fixed lines. Hence the fixed lines of the remaining nine collineations (which do not lie on the quadric considered) are the pairs of lines which join across the intersections of any one of the pairs

$$L_1, L_2;\ M_1, M_2;\ N_1, N_2;$$

with any one of the pairs

$$R_1, R_2;\ S_1,\ S_2;\ T_1,\ T_2.$$

The quadric in question is completely determined by any non-intersecting pair of pairs that lie on it. Of these there are six, while it has been seen that out of the 15 pairs a non-intersecting pair of pairs can be chosen in 60 ways. Hence there are just 10 quadrics on which the 15 pairs of lines lie in sets of six. Moreover, the 15 pairs of lines form the complete intersections of the quadrics in the sense that any two of the quadrics intersect in two pairs of the lines. Thus, of the eight pairs of lines which do not intersect L_1 and L_2, the quadric considered contains two. There are therefore three other quadrics which contain L_1 and L_2. So there are three others which contain M_1 and M_2, and three others which contain N_1 and N_2. Similarly, each of the other nine quadrics must contain either R_1 and R_2, or S_1 and S_2, or T_1 and T_2. The quadric considered must therefore intersect any one of the remaining nine in one pair from L_1 and L_2, M_1 and M_2, N_1 and N_2, and one pair from R_1 and R_2, S_1 and S_2, T_1 and T_2.

4. If instead of starting with a non-intersecting pair of pairs such as L_1 and L_2, M_1 and M_2, one considers the intersecting pair L_1 and L_2, R_1 and R_2, the G_4 which leaves unchanged each of the four lines consists, with identity, of $[L_1, L_2]$, $[R_1, R_2]$ and their product. The fixed lines of their product are the lines joining across the intersections of L_1, L_2, R_1 and R_2; so that the three pairs of fixed lines of the three collineations which, with identity, constitute this G_4, are the pairs of opposite sides of a tetrahedron. Since there are six collineations which leave both L_1 and L_2 unchanged, and two of these occur with L_1 and L_2 in the G_4 in question, there are two other tetrahedra each of which have L_1 and L_2 for a pair of opposite sides; and therefore in all 15 such tetrahedra can be formed from the 15 pairs of lines.

5. Another remarkable property of the 15 pairs of lines is that just six sets of five pairs each may be selected from them so that no two pairs of any set of five intersect; thus L_1 and L_2, M_1 and M_2, and the three pairs which join across the intersections of N_1 and N_2 with R_1 and R_2, S_1 and S_2, T_1 and T_2 respectively form such a set of five. It is, in fact, obvious that no one of the last three pairs intersects L_1 and L_2 or M_1 and M_2. Call P_1 and P_2 the lines which join across the intersections of N_1 and N_2 with R_1 and R_2, and Q_1 and Q_2 those which join the intersections of N_1 and N_2 with S_1 and S_2. If P_1 and P_2 intersect Q_1 and Q_2, then $[P_1, P_2]$ would leave unchanged N_1 and Q_1, so that their point of intersection, *i.e.*, the point of intersection of N_1 and S_1, would be a fixed point for $[P_1, P_2]$. But the only fixed points on N_1 for $[P_1, P_2]$ are the points where R_1 and R_2 meet N_1, so that the point of intersection of N_1 and S_1 is not a fixed point for this collineation. No two of the set of five lines in question then intersect, and they are the only set containing L_1, L_2, M_1, M_2 of which this is true.

Now there are 60 non-intersecting pairs of pairs, from each of which such a set of five will arise, and since any one such set may arise from any one of the $\frac{1}{2}\cdot 5\cdot 4$ pairs contained in it, the number of sets must be six.

III. *On the $G_{16\cdot 720}$.*

6. I go on now to determine the collineations which leave this set of 15 pairs of lines invariant. It may be first noted that the only collineations which change each pair into itself are those of the G_{16}. In fact, the square of any such collineation transforms each line of each pair into itself, and since each pair is met by six others, it must transform every point of each line of each pair into itself. It is, therefore, the identical transformation, and the collineation itself is, therefore, one of order 2. But it has been shown that all collineations of order 2, which are permutable with each collineation of the G_{16}, must be contained in the G_{16}.

It remains only to consider those collineations which permute some or all of the pairs among themselves, and I shall first determine those that permute six pairs lying on one of the 10 quadrics among themselves, say the pairs

$$L_1, L_2;\quad M_1, M_2;\quad N_1, N_2;$$

and

$$R_1, R_2;\quad S_1, S_2;\quad T_1, T_2.$$

Any projective transformation which permutes these among themselves necessarily permutes the remaining nine pairs (which join across the points

of intersection of the six) among themselves, and also transforms the quadric containing the six pairs into itself. Now the projective transformations of space, which leave a quadric invariant, consist of—

(i) Transformations which leave every one of the first set of generators unchanged, while they transform the second set exactly as the points on a line are transformed by the projective transformations of the line; combined independently with those which leave every one of the second set of generators unchanged, and similarly transform the first set; and

(ii) The previous set of transformations compounded with a transformation of order 2 which permutes the two sets of generators.

The most general projective group on a straight line, for which three pairs of points AA', BB', and CC', such that $ABA'B'$, $BCB'C'$, $CAC'A'$ are harmonic, are permuted among themselves, is the well-known G_{24}, ordinarily called the octahedral group.

Hence the most general group for which the three pairs L_1 and L_2, M_1 and M_2, N_1 and N_2 are permuted among themselves, while each of the six lines R_1, R_2, S_1, S_2, T_1, T_2 is unchanged is a G_{24}. Similarly there is a G'_{24} which leaves each of the lines L_1, L_2, M_1, M_2, N_1, N_2 unchanged and permutes the pairs R_1 and R_2, S_1 and S_2, T_1 and T_2 among themselves. It is obvious that the G_{24} and G'_{24} have no common collineations except identity, and that every collineation of G_{24} is permutable with every one of G'_{24}.

Suppose that U_1 and U_2 are the lines which join across the intersections of L_1, L_2 with R_1, R_2, and V_1 and V_2 those that join across the intersections of M_1, M_2 with S_1, S_2, U_1 passing through the intersection of L_1 and R_1, and V_1 through the intersection of M_1 and S_1. Then V_1 and V_2 do not intersect either of the pairs L_1 and L_2, or R_1 and R_2. Hence $[V_1, V_2]$ permutes L_1 and L_2, and also R_1 and R_2; and therefore $[V_1, V_2]$ leaves U_1 and U_2 unchanged. Hence U_1, U_2 and V_1, V_2 are intersecting pairs. Let V_2 and U_2 intersect in O, and consider the perspective of order 2 of which O is the fixed point and the plane of U_1 and V_1 the fixed plane. If V_1 meets U_2 in o, then O, o divide harmonically the two points in which U_2 is met by L_1 and L_2 (or, which is the same thing, by R_2 and R_1). Hence the perspective permutes L_1 with R_1, and L_2 with R_2. Similarly it permutes M_1 with S_1 and M_2 with S_2. The perspective in question, therefore, replaces the two pairs L_1 and L_2, and M_1 and M_2 by the two pairs R_1 and R_2, and S_1 and S_2. Hence it must replace the pair N_1 and N_2, which forms with each of the pairs L_1 and L_2 or M_1 and M_2 a harmonic range on any transversal, by the pair T_1 and T_2, which is similarly related to R_1 and R_2, and S_1 and S_2.

The perspective therefore permutes the two sets of generators on the quadric, and with the G_{24} and G'_{24} generates a G_{1152}, which permutes the six pairs under

consideration among themselves. This group of collineations, which leaves one of the 10 quadrics unchanged, permutes the remaining 9 transitively among themselves. In fact it obviously contains collineations which change any one of the intersecting pairs of pairs, such as L_1, L_2, R_1, R_2, into any other; and the nine intersecting pairs of pairs are, as shown above, the intersections of the invariant quadric with the other nine. There is a similar G_{1152} which leaves any one of the other quadrics invariant. Hence, since the 10 quadrics are transitively permuted by the whole group of collineations for which the 15 pairs of lines are invariant, the order of this group is not less than 11520.

Every collineation which leaves the 15 pairs invariant must permute among themselves the six sets of five non-intersecting pairs that can be formed from them. Each pair occurs in two and only two of these sets. Hence every collineation which leaves each of the six sets unchanged must leave each of the 15 pairs unchanged, and it is shown above that the only collineations for which this is the case are those of the G_{16}. The G_{16} is therefore a self-conjugate sub-group of the total group of collineations for which the 15 pairs of lines is invariant; and in respect of this sub-group the group is isomorphic with a group of permutations of six symbols. Now

$$11520 = 16 \times 720,$$

and 720 is the order of the greatest group which is simply isomorphic with a permutation group of six symbols.

Finally, therefore, the order of the greatest group of collineations for which the 15 pairs of lines forms an invariant configuration is 11520. The group contains a self-conjugate Abelian sub-group of order 16, whose operations are all of order 2; and the factor-group in respect of this self-conjugate sub-group is simply isomorphic with the symmetric group of degree 6.

7. If, in addition to collineations, dualistic transformations of space are admitted, the total group of space-transformations for which the configuration of 15 pairs of lines is invariant, is a group of order 23040, constituted by combining with the group of 11520 collineations a set of 11520 dualistic transformations.

A reciprocation with respect to the quadric containing L_1, L_2, M_1, M_2, N_1, N_2, R_1, R_2, S_1, S_2, and T_1, T_2 is, in fact, a dualistic transformation of order 2, which leaves unchanged each of the lines of the six pairs on the quadric and permutes the lines of each of the remaining nine pairs. This transformation, combined with the G_{16}, gives a G_{32}, Abelian, and having all its operations of order 2, for which each of the 15 pairs is invariant. The 16 dualistic transformations of this G_{32} are obtained by combining each of the

collineations of the G_{16} with the above reciprocation. The nine transformations that arise by combining the collineations whose fixed lines do not lie on the first quadric with the reciprocation are immediately seen to be reciprocations with respect to the other nine quadrics. The remaining six transformations arise by combining $[L_1, L_2]$; $[M_1, M_2]$ ...; $[T_1, T_2]$ with the reciprocation. Now, the effect of $[L_1, L_2]$, followed by the reciprocation, is to permute the lines forming each of five pairs, viz., M_1 and M_2, N_1 and N_2, and the lines joining across the intersections of L_1 and L_2 with R_1 and R_2, S_1 and S_2, T_1 and T_2 respectively; and to leave each of the lines forming the other 10 pairs unchanged. The set of five pairs just written is one of the six sets of five, considered in § 5. Hence, each of the remaining six dualistic transformations permutes the lines of each pair of one of the six sets of five, and leaves unchanged the lines of the remaining 10 pairs. This G_{32} is contained self-conjugately within the group of space transformations which permutes the 15 pairs among themselves; so that, as stated above, the order of this total group must be 23040.

IV. *On the* $G_{16\cdot 60}$.

8. The symmetric group of six symbols has two distinct sets of conjugate sub-groups, isomorphic with the alternating group on five symbols, of order 60. A sub-group of one of these sets permutes the six symbols transitively, one of the other set leaves one symbol unchanged and permutes the other five transitively. Hence the $G_{16\cdot 720}$, which contains the Abelian G_{16} self-conjugately, has two distinct sets of sub-groups of order 16·60, each of which contains the G_{16}, and in respect of it is isomorphic with the alternating group on five symbols. A sub-group of one set permutes transitively among themselves the six sets of five non-intersecting pairs, which can be formed from the fixed lines of the 15 collineations of order 2 contained in the G_{16}. A sub-group of the other set leaves one of the set of five non-intersecting pairs unchanged, and permutes the remaining five.

It is a $G_{16\cdot 60}$ of this second set that is now to be considered in some detail, as leading up to the space-configuration of points, lines, and planes. Let L_1, L_2; M_1, M_2; P_1, P_2; Q_1, Q_2; R_1, R_2 be the five non-intersecting pairs which are permuted by every collineation of the $G_{16\cdot 60}$ selected. Every collineation of the $G_{16\cdot 60}$ is completely specified by the permutation which it gives of these 10 lines. For if the collineations S and T gave the same permutation, then ST^{-1} would leave each of the 10 lines, and therefore, also, each of the fixed lines of the remaining 10 collineations of order 2 of the G_{16} unchanged; and it has been seen above that such a collineation leaves every point of space unchanged.

The permutations corresponding to

$$[L_1, L_2], \dots\dots\dots, [R_1, R_2],$$

are

$$\begin{array}{l}
(M_1\, M_2)\,(P_1\, P_2)\,(Q_1\, Q_2)\,(R_1\, R_2), \\
(L_1\, L_2)\qquad\qquad (P_1\, P_2)\,(Q_1\, Q_2)\,(R_1\, R_2), \\
(L_1\, L_2)\,(M_1\, M_2)\qquad\qquad (Q_1\, Q_2)\,(R_1\, R_2), \\
(L_1\, L_2)\,(M_1\, M_2)\,(P_1\, P_2)\qquad\qquad (R_1\, R_2), \\
(L_1\, L_2)\,(M_1\, M_2)\,(P_1\, P_2)\,(Q_1\, Q_2)\qquad\qquad ;
\end{array}$$

and hence those corresponding to the other 10 collineations of order 2 of the G_{16} are

$$\begin{array}{c}
(L_1\, L_2)\,(M_1\, M_2),\quad (L_1\, L_2)\,(P_1\, P_2),\quad (L_1\, L_2)\,(Q_1\, Q_2),\quad (L_1\, L_2)\,(R_1\, R_2), \\
(M_1\, M_2)\,(P_1\, P_2),\quad (M_1\, M_2)\,(Q_1\, Q_2),\quad (M_1\, M_2)\,(R_1\, R_2), \\
(P_1\, P_2)\,(Q_1\, Q_2),\quad (P_1\, P_2)\,(R_1\, R_2), \\
(Q_1\, Q_2)\,(R_1\, R_2).
\end{array}$$

A sub-group of order 60, isomorphic with the alternating group on five symbols, must give all the even permutations of the five pairs of non-intersectors. Hence an operation of order 3 of such a sub-group leaves each line of two pairs unchanged; *i.e.*, as a permutation of the 10 lines, it leaves four fixed and permutes the other six in two cycles of three each. Now, if the sub-group of order 60 permuted the 10 lines transitively, an operation of order 3 belonging to it would only leave one unchanged; and therefore it must permute them intransitively in two sets of five each. Let

$$L_1, M_1, P_1, Q_1, R_1;\quad L_2, M_2, P_2, Q_2, R_2;$$

be the two sets for one sub-group G_{60} of order 60. Then the permutations of the 10 lines given by the collineations of the G_{60}, are formed by taking any even permutation of the lines of one set and combining it with the corresponding even permutation of those of the other set. In particular the G_{60} is generated by

$$(L_1\, M_1\, P_1\, Q_1\, R_1)\,(L_2\, M_2\, P_2\, Q_2\, R_2),$$

$$(L_1\, M_1)\,(P_1\, Q_1)\,(L_2\, M_2)\,(P_2\, Q_2);$$

and its tetrahedral sub-group which leaves L_1 and L_2 unchanged by

$$(M_1\, P_1)\,(Q_1\, R_1)\,(M_2\, P_2)\,(Q_2\, R_2)$$

and

$$(P_1\, Q_1\, R_1)\,(P_2\, Q_2\, R_2).$$

Further, since the Abelian G_{16} and the G_{60} have no collineation, except the identical one in common, the 960 collineations of the group are given by combining any collineation of the G_{16} with any collineation of the G_{60}.

It may further be noticed that since the $G_{16\cdot 60}$ has no self-conjugate sub-group of index 2, no collineation of order 2 belonging to it can be a perspective.

V. *On the Space-Configuration.*

9. The line L_1 is invariant for the sub-group G_8 of the Abelian G_{16}, which consists of

$$1,\ (M_1\ M_2)(P_1\ P_2),\ (M_1\ M_2)(Q_1\ Q_2),\ (M_1\ M_2)(R_1\ R_2),\ (P_1\ P_2)(Q_1\ Q_2),$$
$$(P_1\ P_2)(R_1\ R_2),\ (Q_1\ Q_2)(R_1\ R_2),\ (M_1\ M_2)(P_1\ P_2)(Q_1\ Q_2)(R_1\ R_2),$$

and for the above tetrahedral G_{12}, generated by

$$(M_1\ P_1)(Q_1\ R_1)(M_2\ P_2)(Q_2\ R_2),$$
$$(P_1\ Q_1\ R_1)(P_2\ Q_2\ R_2).$$

These generate a $G_{8\cdot 12}$ for which L_1 is invariant; and this $G_{8\cdot 12}$ contains a G'_{12}, generated by

$$(P_1\ P_2)(Q_2\ Q_2),$$
$$(P_1\ Q_1\ R_1)(P_2\ Q_2\ R_2),$$

which on L_1 obviously sets up a tetrahedral projective group. Hence the $G_{8\cdot 12}$, for which L_1 is invariant, is multiply isomorphic with a tetrahedral group, and sets up on L_1 a group of projective transformations which contains a tetrahedral group. But the only finite group of projective transformations of a straight line which contains a tetrahedral group, and is simply or multiply isomorphic with a tetrahedral group, is the tetrahedral group itself. Hence the group of projective transformations on L_1, which the $G_{8\cdot 12}$ that leaves L_1 invariant sets up, is a tetrahedral group. This tetrahedral group of projections on L_1 has four sub-groups of order 3; and for each there is a pair of fixed points. If these pairs are a, a'; b, b'; c, c'; d, d'; then, suitably choosing one from each pair, a, b, c, d, are a set of points which are transitively permuted among themselves by the $G_{8\cdot 12}$ for which L_1 is invariant; as also are a', b', c', d'. Both a and a' are therefore invariant for a sub-group of order 24; and each is therefore one of a set of 40 points which are transitively permuted by the $G_{16\cdot 60}$. The plane through L_2 and a is one of a set of 40 planes which are transitively permuted by the $G_{16\cdot 60}$, and so also is the plane through L_2 and a'.

The set of 40 points arising by the collineations of the group from a, and the set of 40 planes arising from L_2a', are the points and planes of the space configuration.

10. No four lines, such as M_1, P_1, Q, R_1 belonging to four distinct pairs can lie on a quadric. For $(L_1\ L_2)(M_1\ M_2)$ being a permutation of the 10

lines given by one of the collineations of the group, it follows that M_2 and therefore also P_2, Q_2, R_2, would lie on the same quadric, which is not the case. There are therefore just two lines meeting M_1, P_1, Q_1, R_1; say S_1 and S_1'; and the configuration of the five non-intersecting pairs, as given in § 5, shows that neither S_1 nor S_1' meets L_1, L_2, M_2, P_2, Q_2, or R_2. The collineation represented by $(P_1\, Q_1\, R_1)\,(P_2\, Q_2\, R_2)$ changes this pair of lines, and therefore each of them, into itself. The point in which S_1 meets M_1 is therefore a fixed point for the collineation $(P_1\, Q_1\, R_1)\,(P_2\, Q_2\, R_2)$. Similarly the points in which S_1 meets P_1, Q_1, R_1 respectively are fixed points for the collineations

$$(Q_1\, R_1\, M_1)\,(Q_2\, R_2\, M_2),\ (R_1\, M_1\, P_1)\,(R_2\, M_2\, P_2),\ (M_1\, P_1\, Q_1)\,(M_2\, P_2\, Q_2),$$

and the four points are permuted transitively among themselves by the tetrahedral group which these collineations of order 3 generate. They must therefore belong either to the set of 40 points which arise from a or to the set which arises from a'. But the two points in which S_1 and S_1' meet M_1 are the fixed points of $(P_1\, Q_1\, R_1)\,(P_2\, Q_2\, R_2)$ which lie on M_1, one of which belongs to the set a, and the other to the set a'. Hence, S_1 may be taken to contain four points of the set a, and S_1' to contain four points of the set a'. Now, the greatest sub-group of the $G_{16\cdot 60}$ for which S_1 is invariant is the above tetrahedral group permuting M_1, P_1, Q_1, R_1. Hence S_1 is one of 80 lines permuted by the $G_{16\cdot 60}$, and on each of them there lie just four points of the set a. These 80 lines with L_1, L_2, M_1, M_2, P_1, P_2, Q_1, Q_2, R_1, R_2, form the set of 90 lines belonging to the configuration, each of which passes through four of the 40 points. Moreover, the 80 lines fall into a set of 40 pairs, which are permuted by the $G_{16\cdot 60}$. Such a pair is formed by S_1 and the line S_2 (intersecting M_2, P_2, Q_2, R_2) into which S_1 is changed by the collineation $(M_1\, M_2)\,(P_1\, P_2)\,(Q_1\, Q_2)\,(R_1\, R_2)$. For this pair is invariant for the sub-group formed by combining the tetrahedral group for which S_1 is invariant with the preceding collineation of order 2. The 90 lines thus fall into 45 pairs which are permuted by the group.

Through each of the 40 points there passes just one of the lines $L_1, L_2, \ldots\ldots, R_1, R_2$. Moreover, the remaining 80 lines, being permuted transitively by the group, and each containing four of the 40 points which are also permuted transitively, 8 of the 80 lines must pass through each point of the 40 points. Hence, in all, just 9 of the 90 lines pass through each of the 40 points.

It may be further noticed that S_1 (and therefore any one of the 80 lines arising from it) contains none of the points of the a' set. For these all lie on the 10 original lines, and the only points in which S_1 meets them belong to the a set.

Further, just 32 of the 80 lines arising from S_1 meet M_1, eight passing through each of the four a-points on M_1. No one of the eight S_1-lines through a given a-point on M_1 meets M_2; and each meets three of the remaining eight lines, L_1, L_2, P_1, P_2, Q_1, Q_2, R_1, R_2 chosen from different pairs. These eight lines contain 32 points of the $\dot{a}$-set, and of these, 24 lie on the eight lines of the S_1-set which pass through a given a-point on M_1.

Hence there are just eight out of the 32 a-points lying on L_1, L_2, P_1, P_2, Q_1, Q_2, R_1, R_2, such that the lines joining them to an assigned a-point on M_1, are not S_1-lines. Since the eight lines L_1, L_2 ...... are permuted transitively by the group of collineations which leaves M_1 invariant, these eight points must lie one on each of the eight lines.

Hence, if a is an a-point, and α, β, γ, δ, four a-points lying on one of the 10 lines, which does not form a pair with that containing a; then of the four lines $a\alpha$, $a\beta$, $a\gamma$, $a\delta$, three are S_1-lines, each containing two other a-points, and the fourth is not an S_1-line.

11. Suppose now that a, b, c, d, are the a-points lying on L_1, and that α is an assigned a-point lying on M_1. The plane $L_1\alpha$ contains the S_1-lines joining α to three out of a, b, c, d; say $b\alpha$, $c\alpha$, $d\alpha$. Each of these contains two other a-points; say, β, γ on $b\alpha$; δ, ϵ on $c\alpha$; ζ, η are $d\alpha$. These six points are the intersections of $L_1\alpha$ with P_1, P_2, Q_1, Q_2, R_1, R_2. Since no S_1-line intersects both lines of an original pair, it may be taken that

$$\beta,\ \gamma,\ \delta,\ \epsilon,\ \zeta,\ \eta$$

lie respectively on $\qquad P_1,\ Q_1,\ P_2,\ R_1,\ Q_2,\ R_2.$

Of the lines joining β to a, c, d, two are S_1-lines, say βa, βc. Each contains two other a-points, not lying on L_1, L_2, M_1, P_1, or P_2, and not being γ. Hence, if βa passes through ϵ, ζ, then βc must pass through η and an a-point θ not belonging to the previous six. This point must, therefore, lie on M_2; and hence the eight points in which $L_1\alpha$ meets M_1, M_2, P_1, P_2, Q_1, Q_2, R_1, R_2, are all a-points. It follows immediately that this plane contains eight S_1-lines: for through each of the eight a-points in the plane, which does not lie on L_1, three such lines can be drawn, and the eight points lie three by three on these lines.

Also through L_1 a set of four planes can be drawn which contain the 32 a-points that do not lie on L_1 or L_2, viz., the four planes through L_1 and the four a-points on M_1 (or on any one of the remaining seven, excluding L_2). These four planes are permuted transitively by the sub-group of order 96 for which L_1 is invariant and therefore, from any one of them, $L_1\alpha$, a set of 40 planes arise which are permuted transitively by the $G_{16\cdot 60}$.

It remains to consider the point a_2 in which $L_1\alpha$ meets L_2. Since it

is one of four points permuted by the sub-group for which L_2 is invariant, it must be an a-point, or an a'-point. If it were an a-point, the collineation $[M_1\ M_2]$ which permutes L_1 and L_2 must permute a_2 with one of the a-points on L_1, say a, changing the plane $L_1\alpha$ (which contains a_2) into the plane L_2a and leaving the line aa_2 unaltered. But $[M_1, M_2]$ leaves every point on M_1 and M_2 unaltered and, therefore, leaves the points α, θ in which $L_1\alpha$ meets M_1 and M_2 unaltered. Hence the line $\alpha\theta$ must coincide with aa_2. Similarly the lines ba_2, ca_2, da_2 must each contain one of the pairs β, δ; γ, ζ; ϵ, η. This is certainly impossible, for it implies that the 12 a-points of $L_1\alpha$ (other than a_2), which have been proved to lie four by four on 9 straight lines, also lie three by three on four concurrent lines. Hence a_2 must be an a'-point; and each of the 40 planes arising from $L_1\alpha$ contains just 12 of the 40 a-points. These 12 points lie four by four on nine lines, viz., the eight S_1-lines in the plane, and the one line in the plane which belongs to the original 10, in other words the nine out of the 90 lines which lie in the plane contain the 12 out of the 40 points four by four. The four of the 40 planes which pass through S_1 each contain just eight a-points other than those on S_1. Hence, in all, the four planes contain 36 of the 40 points. The four remaining ones must be permuted among themselves by the collineations of the tetrahedral sub-group of $G_{16\cdot 60}$ for which S_1 is invariant. But the only sets of four a-points which are permuted by this group are those on L_1, L_2, S_1 and its pair S_2. Hence the four planes of the set through S_1 contain all the a-points except those on S_2. They are, therefore, determined by drawing the four planes through S_1 and the four a-points on any other S_1-line which intersects neither S_1 nor S_2. Now it has been seen that the S_1-lines in any of these planes intersect only in a-points. Further, an S_1-line which does not lie in one of these planes does not intersect S_1 at all. Hence the 40 a-points form the complete intersection of the 80 S_1-lines, and therefore also of the whole set of 90 lines of the configuration.

With each of the 40 a-points one of the 40 planes may be associated in a definite manner. Let a, a' be the fixed points of a collineation of order 3 of the $G_{16\cdot 60}$ on L_1, and consider a and the plane L_2a'. If S_1 is an S_1-line through a it must meet L_2a' in a point s which does not lie on L_2. Now the four planes through S_1 which contain all the 40 points except the four that lie on S_2, the pair of S_1, meet L_2a' in four concurrent lines passing through s. But of the 12 a-points in L_2a' not more than nine can lie on four concurrent lines. Hence of the 12 a-points on L_2a', at least three do not lie on the four planes through S_1 which contain all the a-points, except those of S_2. In other words S_2 lies on L_2a'. Similarly the pair of each S_1-line, which passes through a, lies on L_2a'. Also L_1 passes through a, and L_2 lies on L_2a'. Hence the pair to each

one of the nine lines through α, belonging to the set of 90, lies on $L_2\alpha'$. The point s in which S_1 meets $L_2\alpha'$ is not an α-point, because the 4 of the 40 planes which contain S_2 meet S_1 in points which are not α-points. Hence the α-points on $L_2\alpha'$ are distinct from the α-points on the nine lines which pass through α. Now, there are 12 α-points on $L_2\alpha'$, and $1+3\cdot 9=28$ α-points on the nine lines through α. The nine S_1-lines in $L_2\alpha'$ and the nine S_1-lines through α (their pairs) thus contain the whole system of 40 points.

Since the planes and points are permuted transitively by the group $G_{16\cdot 60}$, 12 of the 40 planes pass through each one of the 40 points and contain the nine lines passing through it in sets of three, four of the planes passing through each of the lines.

12. The complete specification of the configuration that has thus been established is as follows:—

It contains 40 points, 90 lines (forming 45 pairs) and 40 planes. Through each point there pass nine of the lines and 12 of the planes. Through every two of these 9 lines, one of the 12 planes passes, containing a third line; so that the planes contain the lines three by three, and the lines are the intersection of the planes four by four.

In each plane lie 12 of the points and nine of the lines, every two lines intersecting in one of the points through which a third line passes, so that the points lie four by four on the lines and the lines pass three by three through the points.

On each line there lie four of the points, and through each line there pass four of the planes. The four planes through a line contain all the points except the four which lie on a second line which forms a pair with the given one; and through the four points on the line there pass all the planes except the four which pass through the same second line. Further, no two of the lines intersect in a point other than one of the 40 points; and no two lines lie in a plane other than one of the 40 planes.

Moreover, with each of the 40 points may be associated a particular one of the 40 planes, so that the pairs of the nine lines which pass through the point all lie in the plane; while at the same time the nine pairs contain the whole of the 40 points, and the whole of the 40 planes pass through lines belonging to the nine pairs.

The configuration is invariant for a $G_{16\cdot 60}$, which permutes the points and planes, each transitively, while it permutes the lines in two transitive sets of 10 and 80 (5 and 40 pairs).

VI. *The G_{25920} for which the Configuration is Invariant.*

13. There are just 16 S_1-lines, which meet neither L_1 or L_2, and they form eight pairs. Any one of the 16 may be denoted by the symbol $(M_a P_b Q_c R_d)$; $(a, b, c, d = 1 \text{ or } 2)$, M_a, P_b, Q_c, R_d, being the four lines, from the original ten, which it meets; S_1 and S_2 being denoted by the symbols $(M_1 P_1 Q_1 R_1)$ and $(M_2 P_2 Q_2 R_2)$. From the pair S_1, S_2, three other pairs U_1, U_2; V_1, V_2; W_1, W_2 arise by the collineations;—

$$1, (Q_1 Q_2)(R_1 R_2), (R_1 R_2)(P_1 P_2), (P_1 P_2)(Q_1 Q_2),$$

which constitute a sub-group of order 4 of the G_{16}. These pairs are

$$\begin{pmatrix} M_1 P_1 Q_1 R_1 \\ M_2 P_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} M_1 P_1 Q_2 R_2 \\ M_2 P_2 Q_1 R_1 \end{pmatrix}, \begin{pmatrix} M_1 P_2 Q_1 R_2 \\ M_2 P_1 Q_2 R_1 \end{pmatrix}, \begin{pmatrix} M_1 P_2 Q_2 R_1 \\ M_2 P_1 Q_1 R_2 \end{pmatrix}.$$

Similarly, from $\bar{S}_1$ and $\bar{S}_2$—viz., $(M_1 P_1 Q_1 R_2)$, $(M_2 P_2 Q_2 R_1)$—by the collineations of the same sub-group, three other pairs $\bar{U}_1$ $\bar{U}_2$; $\bar{V}_1$, $\bar{V}_2$; $\bar{W}_1$, $\bar{W}_2$ arise, giving another set of 4 pairs, viz.,

$$\begin{pmatrix} M_1 P_1 Q_1 R_2 \\ M_2 P_2 Q_2 R_1 \end{pmatrix}, \begin{pmatrix} M_1 P_1 Q_2 R_1 \\ M_2 P_2 Q_1 R_2 \end{pmatrix}, \begin{pmatrix} M_1 P_2 Q_1 R_1 \\ M_2 P_1 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} M_1 P_2 Q_2 R_2 \\ M_2 P_1 Q_1 R_1 \end{pmatrix}.$$

No two lines of the first set of four pairs (or of the second set) intersect. In fact, if $M_1 P_1 Q_1 R_1$, $M_1 P_1 Q_2 R_2$ intersect, it must be in a point of M_1 or of P_1. But the a-points of both M_1 and P_1 are permuted by the collineation $(Q_1 Q_2)(R_1 R_2)$ which changes $M_1 P_1 Q_1 R_1$ into $M_1 P_1 Q_2 R_2$.

On the other hand, since the eight pairs of the two sets include all the S_1-lines which intersect neither L_1 nor L_2, while each contains four a-points, each of the 32 a-points that do not lie on L_1 or L_2 must lie on two of the 16 lines, and therefore each line of the first set must intersect four lines of the second, one from each pair.

Hence the two sets of four pairs are the only sets of four pairs of S_1-lines which contain all the 32 a-points not lying on L_1 or L_2. From the five original pairs and the 40 pairs of S_1-lines, then, just three sets of five pairs can be chosen so as to include L_1 and L_2, and to contain the whole of the 40 points. These are:—

$$\begin{array}{lllll} L_1 L_2; & M_1 M_2; & P_1 P_2; & Q_1 Q_2; & R_1 R_2; \\ L_1 L_2; & S_1 S_2; & U_1 U_2; & V_1 V_2; & W_1 W_2; \\ L_1 L_2; & \bar{S}_1 \bar{S}_2; & \bar{U}_1 \bar{U}_2; & \bar{V}_1 \bar{V}_2; & \bar{W}_1 \bar{W}_2. \end{array}$$

Similarly with each of the original five pairs, just two sets, formed each with four pairs of S_1-lines, can be constructed so as to contain the whole of the 40 points. This gives in all 11 sets, of five pairs each, from the 45 pairs

of the configuration, such that each set contains all the 40 points, while one pair of each set (all five pairs of one set) belongs to the original five pairs.

14. The five pairs of S_1-lines

$$\begin{pmatrix} M_1 P_1 Q_1 R_1 \\ M_2 P_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_1 P_1 Q_1 R_1 \\ L_2 P_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_1 M_1 Q_1 R_1 \\ L_2 M_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_1 M_1 P_1 R_1 \\ L_2 M_2 P_2 R_2 \end{pmatrix}, \begin{pmatrix} L_1 M_1 P_1 Q_1 \\ L_2 M_2 P_2 Q_2 \end{pmatrix},$$

are non-intersectors. For if $(M_1\ P_1\ Q_1\ R_1)$, $(L_1\ P_1\ Q_1\ R)$ intersect, it must be in a point of P_1 or Q_1 or R_1, and this would mean that two of these lines lie in a plane. These five pairs therefore contain the whole of the 40 points. So also do the five pairs derived from the preceding by the collineation $[L_1\ L_2]$ of the $G_{16\cdot 80}$, viz.:—

$$\begin{pmatrix} M_1 P_1 Q_1 R_1 \\ M_2 P_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_2 P_1 Q_1 R_1 \\ L_1 P_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_2 M_1 Q_1 R_1 \\ L_1 M_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_2 M_1 P_1 R_1 \\ L_1 M_2 P_2 R_2 \end{pmatrix}, \begin{pmatrix} L_2 M_1 P_1 Q_1 \\ L_1 M_2 P_2 Q_2 \end{pmatrix}.$$

Now a given S_1-line meets just 28 other S_1-lines and four of the original 10 lines; and its pair meets the pairs of these. Hence there are just 11 pairs of S_1-lines and one pair of the original 10 lines, which neither of a given S_1 pair intersect. For the pair S_1, S_2 or $(M_1\ P_1\ Q_1\ R_1)$, $(M_2\ P_2\ Q_2\ R_2)$, these 12 pairs are those which occur in the preceding sets of five, and in the set (already obtained).

$$\begin{pmatrix} M_1\ P_1\ Q_1\ R_1 \\ M_2\ P_2\ Q_2\ R_2 \end{pmatrix}, \begin{pmatrix} L_1 \\ L_2 \end{pmatrix}, \begin{pmatrix} M_1\ P_1\ Q_2\ R_2 \\ M_2\ P_2\ Q_1\ R_1 \end{pmatrix}, \begin{pmatrix} M_1\ P_2\ Q_1\ R_2 \\ M_2\ P_1\ Q_2\ R_1 \end{pmatrix}, \begin{pmatrix} M_1\ P_2\ Q_2\ R_1 \\ M_2\ P_1\ Q_1\ R_2 \end{pmatrix}.$$

In each of these sets of five pairs the four pairs after the first contain all the a-points except those lying on S_1 and S_2. Hence any two pairs taken from different sets (excluding S_1 and S_2) are necessarily intersecting pairs. The three sets given are then the only sets of five pairs, including S_1 and S_2, which contain all 40 points.

Combining this with the immediately preceding result, it follows that from the 45 pairs of lines of the configuration, just three sets of five pairs each can be formed, so as to include a given pair and to contain the whole 40 points. The total number of such sets is therefore $3 \times 45/5 = 27$. Of the 27 sets, 11 contain members from a given set, including the set itself, and the remaining 16 have no members from the given set.

Each of these sets of five pairs has the property that the five pairs of lines forming it are the fixed lines of five mutually permutable collineations of order 2. This will first be proved for one of the sets of five containing an original pair, say the set

$$\begin{pmatrix} L_1 \\ L_2 \end{pmatrix}, \begin{pmatrix} S_1 \\ S_2 \end{pmatrix}, \begin{pmatrix} U_1 \\ U_2 \end{pmatrix}, \begin{pmatrix} V_1 \\ V_2 \end{pmatrix}, \begin{pmatrix} W_1 \\ W_2 \end{pmatrix},$$

$$\text{or} \begin{pmatrix} L_1 \\ L_2 \end{pmatrix}; \begin{pmatrix} M_1\ P_1\ Q_1\ R_1 \\ M_2\ P_2\ Q_2\ R_2 \end{pmatrix}, \begin{pmatrix} M_1\ P_1\ Q_2\ R_2 \\ M_2\ P_2\ Q_1\ R_1 \end{pmatrix}, \begin{pmatrix} M_1\ P_2\ Q_1\ R_2 \\ M_2\ P_1\ Q_2\ R_1 \end{pmatrix}, \begin{pmatrix} M_1\ P_2\ Q_2\ R_1 \\ M_2\ P_1\ Q_1\ R_2 \end{pmatrix}.$$

Each of the pair L_1 and L_2 is invariant for a tetrahedral sub-group of the $G_{16\cdot 60}$, which contains the three collineations of order 2.

$$(M_1 P_1)(Q_1 R_1)(M_2 P_2)(Q_2 R_2), \quad (M_1 Q_1)(P_1 R_1)(M_2 Q_2)(P_2 R_2),$$
$$(M_1 R_1)(P_1 Q_1)(M_2 R_2)(P_2 Q_2),$$

say
$$A,\ B,\ AB;$$

and the collineation of order 3,

$$(P_1 Q_1 R_1)(P_2 Q_2 R_2), \text{ say } C.$$

The permutations of the 10 lines forming the set considered given by these collineations are

$$A \sim (V_1 V_2)(W_1 W_2),$$
$$B \sim (U_1 U_2)(W_1 W_2),$$
$$AB \sim (U_1 U_2)(W_1 W_2),$$
$$C \sim (U_1 V_1 W_1)(U_2 V_2 W_2).$$

From this it follows that L_1, L_2, S_1, S_2, U_1, U_2 cannot lie on a quadric; for, in virtue of the collineation C, it would also contain V_1, V_2, W_1, W_2; and the 40 points do not lie on a quadric. On the other hand, since the fixed lines of A, B and AB meet L_1, L_2, S_1, S_2, the four latter are generators of one system of a quadric, of which the fixed lines of A, B and AB, are generators of the other system. Denoting, as usual, the fixed lines of A, B, and AB, by A_1, A_2; B_1, B_2; $(AB)_1$, $(AB)_2$; U_1 and U_2 must meet both A_1 and A_2, since the collineation A leaves U_1 and U_2 unchanged. Similarly V_1 and V_2 meet both B_1 and B_2; while W_1 and W_2 meet $(AB)_1$ and $(AB)_2$. Further, since the collineation B leaves every generator of the first system unaltered, while it permutes A_1 with A_2 and U_1 with U_2, the points of intersection of A_1 with U_1, and of A_2 with U_2 must lie on a generator of the first system. Now the collineation C leaves the quadric and every generator of the first set unchanged, while it permutes cyclically the pairs A_1, A_2; B_1, B_2; $(AB)_1$, $(AB)_2$ of the other system. It also permutes cyclically the pairs U_1, U_2; V_1, V_2; W_1, W_2. Hence U_1, U_2; V_1, V_2; W_1, W_2 must join across the intersections of A_1, A_2; B_1, B_2; $(AB)_1$, $(AB)_2$ with a common pair of generators X_1, X_2 of the first system.

Hence

$$[U_1 U_2] = [A_1 A_2][X_1 X_2],$$
$$[V_1 V_2] = [B_1 B_2][X_1 X_2],$$
$$[W_1 W_2] = [(A B)_1 (A B)_2][X_1 X_2].$$

But $[X_1 X_2]$ is permutable with $[A_1 A_2]$, $[B_1 B_2]$, $[(A B)_1 (A B)_2]$, which are permutable among themselves. Hence $[U_1 U_2]$, $[V_1 V_2]$, $[W_1 W_2]$ are mutually

permutable. Now from their mode of formation the pairs S_1, S_2; U_1, U_2; V_1, V_2; W_1, W_2; are permuted among themselves by the collineations of the group

$$1, \quad (Q_1\, Q_2)(R_1\, R_2), \quad (R_1\, R_2)(P_1\, P_2), \quad (P_1\, P_2)(Q_1\, Q_2).$$

Hence $[S_1\, S_2]$, $[U_1\, U_2]$, $[V_1\, V_2]$, $[W_1\, W_2]$ are all mutually permutable; and since $[L_1\, L_2]$ permutes the lines of each pair it is permutable with each of them.

Consider now the plane $S_1\alpha$ through S_1 and an α-point on U_1. The remaining α-points on it are the points where it is met by L_1, L_2, U_2, V_1, V_2, W_1, W_2. The collineation $[S_1\, S_2]$ leaves this plane unchanged, since it leaves every point on S_2 unchanged. Moreover it permutes the pairs L_1, L_2; U_1, U_2; V_1, V_2; W_1, W_2. Hence it permutes the α-points in which $S_1\alpha$ is met by the four pairs. Similarly $[S_1\, S_2]$ permutes the α-points in the planes $S_1\beta$, $S_1\gamma$, $S_1\delta$; β, γ, δ being the other α-points on U_1. Further $[S_1\, S_2]$ leaves unchanged the eight α-points on S_1 and S_2. Hence $[S_1\, S_2]$ permutes among themselves the 40 α-points; and the same is true for the collineation of order 2, whose fixed lines are any one of the 45 pairs.

15. Consider next any one of the sets of five line-pairs which contain all the 40 points. If X_1, X_2 is any line pair of the 45, $[X_1, X_2]$ changes the five line pairs in question into another set of five which contain the 40 points. By suitably choosing X_1, X_2 it may be ensured that one of the new pairs belongs to the original five pairs. Then by the preceding paragraph the five collineations of order 2 of which the five new pairs are the fixed lines are mutually permutable; and therefore the same is true for the five pairs from which they were transformed. The five collineations of order 2, of which the five pairs are the fixed lines, generate an Abelian G_{16}. In fact, in an Abelian G_8, five collineations of order 2 cannot be chosen so that all their fixed lines are non-intersectors; and, on the other hand, there is no Abelian G_{32} of collineations of order 2. The Abelian G_{16} is contained self-conjugately in a $G_{16\cdot 60}$, for which the set of five pairs in question, and the 40 points of the configuration, are invariant. There are thus just 27 $G_{16\cdot 60}$'s for which the configuration is invariant. Each of these leaves one set of five invariant, and permutes the remaining 26 in two sets of 10 and 16. In fact, the $G_{16\cdot 60}$ for which the original set of five pairs is invariant must permute among themselves the set of 10 (§ 13), each of which contains one of the original five. Now the $G_{16\cdot 60}$ permutes the five pairs L_1, L_2, ..., R_1, R_2 transitively, and the collineation $[M_1\, M_2]$ clearly permutes the two sets (§ 13),

$$L_1\, L_2; \quad S_1\, S_2; \quad U_1\, U_2; \quad V_1\, V_2; \quad W_1\, W_2;$$

and

$$L_1\, L_2; \quad \overline{S}_1\, \overline{S}_2; \quad \overline{U}_1\, \overline{U}_2: \quad \overline{V}_1\, \overline{V}_2; \quad \overline{W}_1\, \overline{W}_2.$$

Hence these 10 sets are permuted transitively by the $G_{16\cdot 60}$. The remaining 16 sets are permuted transitively by the Abelian G_{16}, which is contained self-conjugately in the $G_{16\cdot 60}$ in question. In fact, the 16 sets into which the set of § 14, viz.:—

$$\begin{pmatrix} M_1 P_1 Q_1 R_1 \\ M_2 P_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_1 P_1 Q_1 R_1 \\ L_2 P_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_1 M_1 Q_1 R_1 \\ L_2 M_2 Q_2 R_2 \end{pmatrix}, \begin{pmatrix} L_1 M_1 P_1 R_1 \\ L_2 M_2 P_2 R_2 \end{pmatrix}, \begin{pmatrix} L_1 M_1 P_1 Q_1 \\ L_2 M_2 P_2 Q_2 \end{pmatrix},$$

are changed by the collineations of the original G_{16} are all distinct.

The 27 sets of five pairs then are permuted transitively by the greatest group of collineations for which the configuration is invariant, so that the order of this group must be equal to or a multiple of 27.16.60. Now any collineation which leaves each of the 27 sets unchanged must also leave each of the 45 line-pairs unchanged, and is certainly the identical collineation; *i.e.*, that which leaves every point of space unchanged.

Moreover, any collineation which leaves a single set unchanged must contain the corresponding Abelian G_{16} self-conjugately, and must therefore be a sub-group of the $G_{16\cdot 720}$ of § 6. The only sub-group of this greater than and containing the $G_{16\cdot 60}$ for which the five line-pairs are invariant, is a $G_{16\cdot 120}$, which in respect of the Abelian G_{16} is simply isomorphic with the symmetric group of five symbols. For this group, however, it may be readily verified that the points called a and a' are conjugate (the group of transformations effected on L_1 by the sub-group for which L_1 is invariant, being the octohedral and *not* the tetrahedral group), so that a is one of a set of 80 points permuted by the group. Hence the $G_{16\cdot 60}$ is the greatest group of collineations for which the 40 points and a set of five-lines-pairs is invariant.

It follows, therefore, that the order of the greatest group of collineations for which the configuration is invariant is $27.16.60 = 2^6.3^4.5$.

16. It might be anticipated that if in addition to collineations dualistic transformations were admitted, the order of the group for which the configuration is invariant would be doubled, and this is, in fact, the case.

As in § 7, an ordinary reciprocation with respect to the quadric containing L_1, L_2, M_1, M_2 leaves each of these lines unaltered, and permutes each of the pairs P_1, P_2; Q_1, Q_2; R_1, R_2. Now the $G_{16\cdot 60}$, of § 8, is contained in a $G_{16\cdot 120}$ which permutes the a-points with the a'-points, and to which belongs the collineation that permutes L_1 with M_2, L_2 with M_1, P_1 with P_2, Q_1 with Q_2, and R_1 with R_2. The reciprocation followed by this collineation is a dualistic transformation giving the permutation $(L_1\ M_2)\ (L_2\ M_1)$ of the 10 lines. This dualistic transformation is permutable with the collineation $(P_1\ Q_1\ R_1)$ $(P_2\ Q_2\ R_2)$. The latter leaves every point of two generators K_1, K_2

of the quadric containing L_1, L_2, M_1, M_2, belonging to the system opposite to L_1, unchanged. Of these one, say K_1, meets L_1 in an α-point. Now the dualistic transformation changes the point of intersection of K_1 and L_1 into the plane containing K_1 and M_2. The point of intersection of K_1 and L_1 is one of the 40 points of the configuration; and the plane through M_2 and K_1, *i.e.*, through M_2 and an α-point on L_1, is one of the 40 planes. The configuration, therefore, is invariant for the dualistic transformation.

VII. *Identification of the* G_{25920} *with a known Simple Group.*

17. The identification of the G_{25920} of collineations thus arrived at with a known group is readily effected from a consideration of the 27 sets of five pairs, formed from the 45 pairs of lines of the collineation which are permuted by the collineations of the group. For this purpose a rather more convenient notation for the pairs is introduced. Any pair is, in fact, adequately represented by such a symbol as—

$$(a,\ b,\ c,\ d,\ e),$$

where of the five letters either one or four are zero, while the others are either 1 or 2. Thus, with this notation, the pair L_1, L_2 would be represented either by (1, 0, 0, 0, 0) or (2, 0, 0, 0, 0); while the pair L_1, $P_2\,Q_1$, R_1, $L_2\,P_1$, $Q_2\,R_2$ is represented either by (1, 0, 2, 1, 1) or (2, 0, 1, 2, 2). Either form is derived from the other by multiplying each of the numbers by 2 and then reducing mod-3; and there is no risk of confusion between them.

With this notation the 27 sets of five are given in the following table. The first 11 are those which have a common element with the first, constructed as in §13; and the remaining 16 are those having no common element with the first; and formed by carrying out the operations of the Abelian G_{16}, for which each pair of the first set is invariant on any one of the 16, such as that given at the beginning of §14.

10000	01000	00100	00010	00001	*a*
10000	01111	01122	01212	01221	*b*
10000	01112	01121	01211	02111	*c*
00001	11110	11220	12120	12210	*d*
00001	11120	11210	12110	21110	*e*
01000	10111	10122	10212	10221	*f*
01000	10112	10121	10211	20111	*g*
00100	11011	11022	12012	12021	*h*
00100	11012	11021	12011	21011	*i*
00010	11101	11202	12102	12201	*k*
09010	11102	11201	12101	21101	*l*
01111	10111	11011	11101	11110	*m*
01111	20111	21011	21101	21110	*n*
01112	10112	11012	11102	11110	*m'*
01112	10221	12021	12201	21110	*n'*
01221	10221	11021	11201	11220	*p*
01221	10112	12012	12102	12110	*q*
02111	20111	11022	11202	11220	*p'*
02111	10111	12011	12101	12110	*q'*
01212	10121	12021	12101	12120	*r*
01212	10212	11012	11202	11210	*s*
01211	10122	21011	12102	12120	*r'*
01211	10211	11011	11201	11210	*s'*
01122	10211	12011	12201	12210	*t*
01122	10122	11022	11102	11120	*u*
01121	10212	12012	21101	12210	*t'*
01121	10121	11021	11101	11120	*u'*

From these 27 sets 45 triplets such as *abc* can be formed, each consisting of the three sets of five which contain one of the 45 pairs. These triplets are—

abc, *ade*, *afg*. *ahi*, *akl*, *bmn*, *bpq*, *brs*, *btu*,
cm'n' *cp'q'* *cr's'*, *ct'u'*, *dmm'*, *dpp'*, *drr'*, *dtt'*, *enn'*,
eqq', *ess'*, *euu'*, *fmq'*, *fpn'*, *fst'*, *fr'u*, *gm'q*, *gp'm*,
gs't, *gru'*, *hms'*, *hn'r*, *hp'u*, *hqt'*, *im's*, *inr'*, *ipu'*,
iq't, *kmu'*, *kn't*, *kp's*, *kqr'*, *lm'u*, *lnt'*, *lps'*, *lq'r*;

where the letters denote the 27 sets of five as in the table. Every collineation of the group permutes the 27 sets, and therefore gives a permutation of the 27 letters, and these permutations must be such as also

to permute the 45 triplets among themselves. But in this form the group presents itself in connection with the 27 lines on a cubic surface. The earliest investigation of the group connected with this problem is due to M. Jordan.* M. Jordan shows that if the 27 letters a, b, ... (the letter-notation has been chosen to agree with his) denote the 27 lines on a cubic surface, then the 45 triplets of the preceding set give the 45 triangles which can be formed from them. He further shows that the most general group of permutations of the 27 letters for which the set of triplets is invariant, is a group of order 2×25920, which contains a single sub-group (necessarily self-conjugate) of order 25920, and that this latter group is a simple group. The G_{25920} of collineations for which the configuration of points, lines, and planes is invariant is therefore a simple group isomorphic with the known simple group of the same order on which the determination of the 27 lines on a cubic surface depends.

VIII. *Some Properties of the G_{25920}.*

18. The 45 collineations of order 2 whose fixed lines are the 45 pairs of lines of the configuration, form a single conjugate set of collineations in the G_{25920}; for the group contains collineations which change any one of the 27 sets of five into any other, and in a $G_{16\cdot 60}$, for which one set of five is invariant, there are collineations changing any one pair of the five into any other.

Any one of these 45 collineations has been seen to be permutable with just 12 others, and the product of two such permutable collineations A and B is another collineation I of order 2. The fixed lines of this collineation lie on the quadric which contain the fixed lines of A and B. But it follows, from the construction of the S_1-lines, that no three non-intersecting pairs of lines chosen from the 45 can lie on a quadric. Hence I cannot be conjugate to A. Suppose now that the product of two other permutable collineations C and D of the set of 45 were I. Then the fixed lines of I would lie on two quadrics containing respectively the fixed lines of A, B and of C, D. There would therefore be two lines meeting these eight fixed lines, which is not the case. Hence I is one of a set of 270 collineations of order 2; in fact, there are 12×45 collineations of the form AB, and I occurs just twice, viz., in the forms AB and BA.

Consider next two collineations of the 45 which are not permutable, say those of which L_1, L_2 and $L_1M_1P_1Q_1$, $L_2M_2P_2Q_2$ or X_1, X_2 are the fixed lines. Denote them by L and X, and the points of intersection of L_1, X_1

* 'Traité des Substitutions,' pp. 316—329.

and L_2, X_2 by a_1, a_2. Both L and X leave a_1, a_2 and the planes $L_1 X_1$, $L_2 X_2$ unchanged. Hence LX leaves every point of $a_1 a_2$ unchanged. Also L must change X_1 into another S_1-line through a_1 in the plane $L_1 X_1$, and similarly X changes L_1 into another S_1-line through a_1 in $L_1 X_1$. But there is only one other such line (§ 12). Hence

$$XLX = LXL,$$

or LX is a collineation of order 3. If Y denotes XLX, then this collineation of order 3 can be written in the three forms LX, XY, YL. Suppose now that there were other collineations of the 45, such that

$$L'X' = LX;$$

then both L′ and X′ would change $a_1 a_2$ into itself, so that $a_1 a_2$ would meet the eight fixed lines of L, X, L′, X′. This cannot be the case, and there are therefore no such collineations as L′ and X′. Hence LX is one of 480 distinct collineations. In fact there are 32×45 collineations of the forms LX, and any given one occurs just three times. This set includes with each collineation its inverse.

The line $a_1 a_2$ is one of 240, which are permuted by the group; for there are 16 lines joining any a-point on L_1 to any a-point on L_2, and of the 16×45 that so arise from the 45 pairs each occurs three times. Moreover, the sub-group for which a_1 is invariant leaves the plane $L_2 a_1'$ invariant and permutes the 12 a-points in this plane transitively. Hence the 240 lines arising from $a_1 a_2$ are permuted transitively by the group. Each of these lines is an absolutely fixed line for a collineation of order 3 and its inverse. Hence the set of 480 collineations of order 3, such as LX, is a single conjugate set.

Also the set of 270 collineations of order 2 to which I belongs is a single conjugate set. For it follows immediately, from the fact that LX is of order 3 when L and X are not permutable, that the 12 collineations of the 45 that are permutable with a given one, form a single conjugate set in the sub-group containing that given one self-conjugately.

The two conjugate sets of 45 and 270 collineations of order 2 are the only ones in the group. Any collineation of order 2 must transform some set of five pairs, say

$$L_1, L_2;\quad M_1, M_2;\quad P_1, P_2;\quad Q_1, Q_2;\quad R_1, R_2,$$

into itself. If it transforms each pair into itself, it belongs to the corresponding Abelian G_{16}, and is therefore either one of the 45 or one of the 270. If it permutes the sets it may be taken to be

$$(M_1 P_1)(Q_1 R_1)(M_2 P_2)(Q_2 R_2);$$

but the effect of this collineation on the five pairs

$$L_1, L_2; \quad S_1, S_2; \quad U_1, U_2; \quad V_1, V_2; \quad W_1, W_2,$$

is to transform each pair into itself. Hence, again, the collineation belongs either to the 45 or to the 270.

Another remarkable set of collineations belonging to the G_{25920} are the perspectives of order 3. It has been seen in § 12 that the 40 points all lie either on the nine lines through a or in the plane L_2a'. If any S_1-line through a meet L_2a' in α, then a, α are the fixed points of a collineation of order 3 on the line which leaves a unchanged and permutes the remaining three a-points on the line. Hence a perspective of order 3, of which a is the fixed point and L_2a' the fixed plane, permutes the 40 a-points, leaving 13 unchanged. The configuration being invariant for this perspective, it must belong to the G_{25920}. There are, then, 80 perspectives, each of order 3, having the 40 points and corresponding 40 planes for their fixed points and planes belonging to the group. No perspective can be transformed into its inverse by a collineation; so that these 80 perspectives fall into two conjugate sets of 40 each.

19. From the 27 sets of five pairs it is possible to choose 12 which contain each of 30 pairs twice and none of the remainder. Assuming the possibility of such a choice, if a, in the table of § 17, is taken for one set, then five others must come from the first 11 sets and the remainder from the other 16. Now the $G_{16\cdot 60}$ for which a is invariant permutes the last 16 sets transitively. Any one of them may therefore be taken with a. If a particular one, m, is chosen, then among the last 16 there are only five others which have a pair in common with m, viz., n, q', s', u', m'. These, then, necessarily belong to the 12. The symbols of the pairs contained in these sets have either no 2 or only one. Hence the remaining five to be chosen from the first 11 must satisfy this condition; and on a reference to the table it is seen that this can be done in just one way, viz., by taking c, g, i, l, c. The set then is uniquely determined by a and m. Of these, a can be chosen in 27 ways, and then m in 16. There are therefore $27 \times 16/12 = 36$ such sets of twelve, and they are permuted transitively by the collineations of the group. The sub-group which leaves one such set invariant is of order 720. Now both a and m are invariant for the G_{60} of § 8, generated by the collineations

$$(L_1\, M_1\, P_1\, Q_1\, R_1)\,(L_2\, M_2\, P_2\, Q_2\, R_2),$$

$$(L_1\, M_1)\,(P_1\, Q_1)\,(L_2\, M_2)\,(P_2\, Q_2).$$

Hence the pair of sets a, m is one of either 6 or 12 pairs, permuted transitively by the G_{720} for which the set of twelve is invariant. If it

were one of 12 there would be two pairs containing a, and their other members would be either invariant for or permuted by the G_{60}. Now the G_{60} is simple, and hence each other member would be invariant for the G_{60}. But a and m are the only two members of the set of twelve which are invariant for the G_{60}. Hence the pair a, m is invariant for a G_{120} and is one of six such pairs for the G_{720}, and the remaining five pairs are c, n; g, q'; i, s'; l, u'; e, m', these being permuted by the G_{60}. A collineation which leaves each of these pairs invariant is found to leave each of the 30 pairs of lines which enter in the set of twelve invariant, and is therefore the identical collineation. Hence the G_{720} is simply isomorphic with a permutation group on 6 symbols. It is therefore the symmetric group in six symbols, and the G_{120} for which the pair a, m is invariant is the symmetric group of five symbols.

Moreover, the groups arrived at thus are the only sub-groups of the G_{25920} simply isomorphic with the symmetric group of the 6 symbols.* Any such sub-group must permute the 27 sets of five intransitively, in groups of degree 6, 10, 12 or 15. The only possible combinations are 12, 15, the case considered above; or 6, 6, 15. Now if six sets of five undergo the permutations of the symmetric group of 6 symbols, each pair of sets must contain a common line-pair, or else no pair of sets contains a common line-pair. A reference to the table shows that neither case is possible.

By an extension of the above reasoning it may be shown that for any sub-group of the G_{25920} which permutes the 27 sets of five intransitively, the smallest number of sets transitively permuted among themselves must be 1, 2, 3, 9 or 12. A set of nine contains each of six pairs of lines three times, and 27 other pairs each once. Such a set is

$$\begin{array}{lll} a, & b, & c, \\ d, & m, & m', \\ e, & n, & n'. \end{array}$$

20. For the sub-groups of the G_{25920} of indices 40 and 45 there are simple geometrical invariants; that for the sub-groups of index 40 consisting of one of the 40 points and its associated plane; while one of the 45 pairs of lines of the configuration is invariant for a sub-group of index 45. For the sub-groups of indices 27 and 36 there are no such simple invariants.

For the $G_{16 \cdot 60}$ of index 27, the corresponding 5 line pairs form an invariant figure; these are the non-intersecting fixed lines of five mutually permutable collineations of order 2. If P_0 is any point of space, and P_1, P_2, P_3, P_4, P_5, the points with which P_0 is changed by the five collineations

* *Cf.* Dickson, 'Lond. Math. Soc. Proc.,' vol. 1, New Series, p. 283.

of order 2, then P_0, P_1, P_2, P_3, P_4, P_5 lie in a plane; this is, in fact, a well-known property of what is called the 16_6 configuration. The five mutually permutable collineations of order 2 then co-ordinate with every point of space a definite plane passing through it; *i.e.*, they define a linear complex, and this complex is invariant for the $G_{16\cdot 60}$. To the 27 sub-groups conjugate with the $G_{16\cdot 60}$ there correspond 27 linear complexes which are permuted by the collineations of the group. These complexes are such that from them may be formed 45 sets of three, such that for each set of three the complex planes corresponding to the same point meet in a line; or in other words, such that the three of a set are not linearly independent. This property is characteristic, and enables the 27 complexes to be constructed from any six which are linearly independent. To effect this determination I represent the complexes which correspond to the 12 sets of five of the preceding paragraph as follows:—

$$\begin{matrix} a, & c, & g, & i, & l, & e, \\ A_0, & A_1, & A_2, & A_3, & A_4, & A_5, \end{matrix} \qquad \begin{matrix} m, & n, & q', & s', & u', & m', \\ B_0, & B_1, & B_2, & B_3, & B_4, & B_5; \end{matrix}$$

and I denote the fact that C can be expressed linearly in terms of A and B by the notation

$$C \sim AB;$$

while the condition that a greater number of linear complexes are not independent is represented by

$$A, B, C, D, \ldots \sim 0.$$

Then if the remaining complexes are denoted by the letters used for the corresponding sets of five, the 45 linear relations between the complexes are given by the following tables:—

$$\begin{array}{lll} b \sim A_0\,A_1 \sim B_0\,B_1, & p' \sim A_1\,B_2 \sim A_2\,B_1, & r \sim A_2\,B_4 \sim A_4\,B_2, \\ f \sim A_0\,A_2 \sim B_0\,B_2, & r' \sim A_1\,B_3 \sim A_3\,B_1, & q \sim A_2\,B_5 \sim A_5\,B_2, \\ h \sim A_0\,A_3 \sim B_0\,B_3, & t' \sim A_1\,B_4 \sim A_4\,B_1, & p \sim A_3\,B_4 \sim A_4\,B_3, \\ k \sim A_0\,A_4 \sim B_0\,B_4, & n' \sim A_1\,B_5 \sim A_5\,B_1, & s \sim A_3\,B_5 \sim A_5\,B_3, \\ d \sim A_0\,A_5 \sim B_0\,B_5, & t \sim A_2\,B_3 \sim A_3\,B_2, & u \sim A_4\,B_5 \sim A_5\,B_4, \end{array}$$

are the first 30. With suitable weights to the complexes, these relations involve

$$A_0+B_0 = A_1+B_1 = A_2+B_2 = A_3+B_3 = A_4+B_4 = A_5+B_5 = C, \text{ say.}$$

Hence
$$B_i = C-A_i;$$

and in terms of the A's b, f, h, k, d, are given by

$$A_0-A_1, \quad A_0-A_2, \quad A_0-A_3, \quad A_0-A_4, \quad A_0-A_5.$$

The remaining 15 relations consist of five sets of three of the form

$$-A_0+A_1,\ A_2,\ A_2,\ B_4,\ B_5 \sim 0,$$

$$-A_0+A_2,\ A_1,\ A_3,\ B_4,\ B_5 \sim 0,$$

$$-A_0+A_3,\ A_1,\ A_2,\ B_4,\ B_5 \sim 0.$$

These can only be satisfied by

$$2C = -A_0+A_1+A_2+A_3+A_4;$$

and then the previous relations determine the remaining 16 complexes uniquely.

The complete set of 27 which verify the required relations are then given in terms of six independent ones (as A_0, A_1, A_2, A_3, A_4, A_5 certainly are) as follows:—

$$A_0,\quad A_1,\quad A_2,\quad A_3,\quad A_4,\quad A_5;\quad (6)$$

$$\begin{array}{ll} -3A_0+A_1+A_2+A_3+A_4+A_5, & -A_0-A_1+A_2+A_3+A_4+A_5, \\ -A_0+A_1-A_2+A_3+A_4+A_5, & -A_0+A_1+A_2-A_3+A_4+A_5, \\ -A_0+A_1+A_2+A_3-A_4+A_5, & -A_0+A_1+A_2+A_3+A_4-A_5;\quad (6) \end{array}$$

$$A_0-A_1,\quad A_0-A_2,\quad A_0-A_3,\quad A_0-A_4,\quad A_0-A_5;\quad (5)$$

and
$$-A_0-A_1-A_2+A_3+A_4+A_5,$$

with nine similar sets, each having three plus and three minus signs (10).

The sub-group of index 36 and order 720, considered in § 19, permutes transitively the six following pairs of the 27 linear complexes, viz.,

$$\begin{array}{ll} A_0, & -3A_0+A_1+A_2+A_3+A_4+A_5; \\ A_1, & -A_0-A_1+A_2+A_3+A_4+A_5; \\ A_2, & -A_0+A_1-A_2+A_3+A_4+A_5; \\ A_3, & -A_0+A_1+A_2-A_3+A_4+A_5; \\ A_4, & -A_0+A_1+A_2+A_3-A_4+A_5; \\ A_5, & -A_0+A_1+A_2+A_3+A_4-A_5. \end{array}$$

Now there is clearly just one linear complex, viz.,

$$-A_0+A_1+A_2+A_3+A_4+A_5,$$

which can be expressed linearly in terms of each of the six pairs that are permuted by the sub-group. Hence the sub-group must leave this linear complex invariant. The simplest geometrical invariant for the sub-groups of index 36 is therefore a linear complex.*

* *Cf.* H. Burkhardt, "Hyperelliptische Modulfunctionen III," 'Math. Ann.,' vol. 41, pp. 321—326.

*On Metallic Reflection and the Influence of the Layer of Transition.**

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(Communicated by Professor J. Larmor, Sec. R.S. Received January 11,—Read January 25, 1906.)

It is well known that when light is propagated in an absorbing medium, the dynamical equations and the boundary conditions are of exactly the same form as for a transparent medium. From a mathematical point of view the only difference between the two cases is that μ, the refractive index in a transparent medium, is replaced in the absorbing medium by a complex quantity $\mu - ia$, where μ is the "refractive index" of the medium, *i.e.*, the ratio of the velocity of light in air to that in the medium, and a is the coefficient of absorption.

When dealing with the problem of reflection we shall take the plane of xy as that of incidence, and $x = 0$ as the surface of separation of the two media; the vectors representing the displacements will then be of the forms $e^{ipt-i(x\cos\phi+y\sin\phi)/V}$ in the incident, and $re^{ipt+i(x\cos\phi-y\sin\phi)/V}$ in the reflected wave. Here ϕ is the angle of incidence for the frequency, and V the velocity of propagation in the first medium. The incident wave is of unit amplitude, and if $r = Re^{i\theta}$, then R and θ represent the amplitude and change of phase in the reflected wave.

When the incident light is polarised perpendicularly to the plane of incidence, r is given by Fresnel's formula

$$r_1 = \frac{\mu\cos\phi - \cos\phi'}{\mu\cos\phi + \cos\phi'} = \frac{\tan(\phi - \phi')}{\tan(\phi + \phi')},$$

where ϕ' is the angle of refraction. For light polarised parallel to the plane of incidence we have the corresponding formula

$$r_2 = \frac{\cos\phi - \mu\cos\phi'}{\cos\phi + \mu\cos\phi'} = \frac{-\sin(\phi - \phi')}{\sin(\phi + \phi')}.$$

When applying these formulæ to the problem of metallic reflection, we have to replace μ by the complex $\mu - ia = Me^{-i\alpha}$. Then ϕ' becomes complex,

[* With regard to the paper "On Newton's Rings formed by Metallic Reflection" ('Roy. Soc. Proc.,' A, vol. 76, 1905, p. 515), of which the proofs were corrected in England, in order to avoid delay, Professor Maclaurin writes:—"I have in one place inadvertently put μ_0 instead of the complex $\mu\ (= \mu_0 - ia)$. Unfortunately this affects some of the results that follow. The errors thus introduced are not numerous, and they do not at all affect the general trend of the argument."]

$\sin\phi = (\mu - i a)\sin\phi'$, and $\cos\phi' = c e^{-iu} = (1 - M^{-2}\sin^2\phi \, . \, e^{2i\alpha})^{\frac{1}{2}}$. With this notation we get

$$r_1 = R_1 e^{i\theta_1} = \frac{M\cos\phi \, . \, e^{-i(\alpha-u)} - c}{M\cos\phi \, . \, e^{-i(\alpha-u)} + c};$$

i.e.,
$$R_1 e^{i\theta_1} = \frac{M\cos\phi\cos(\alpha-u) - c - iM\cos\phi\sin(\alpha-u)}{M\cos\phi\cos(\alpha-u) + c - iM\cos\phi\sin(\alpha-u)}.$$

Whence we get

$$R_1^2 = \frac{M^2\cos^2\phi + c^2 - 2Mc\cos\phi\cos(\alpha-u)}{M^2\cos^2\phi + c^2 + 2Mc\cos\phi\cos(\alpha-u)} = \frac{1-x_1}{1+x_1},$$

where
$$x_1 = \frac{2Mc\cos\phi\cos(\alpha-u)}{M^2\cos^2\phi + c^2},$$

and
$$\tan\theta_1 = \frac{2Mc\cos\phi\sin(\alpha-u)}{M^2\cos^2\phi - c^2}.$$

Similarly,

$$r_2 = R_2 e^{i\theta_2} = \frac{\cos\phi - Mce^{-i[\alpha+u]}}{\cos\phi + Mce^{-i[\alpha+u]}} = \frac{\cos\phi - Mc\cos(\alpha+u) + iMc\sin(\alpha+u)}{\cos\phi + Mc\cos(\alpha+u) - iMc\sin(\alpha+u)},$$

whence

$$R_2^2 = \frac{M^2c^2 + \cos^2\phi - 2Mc\cos\phi\cos(\alpha+u)}{M^2c^2 + \cos^2\phi + 2Mc\cos\phi\cos(\alpha+u)} = \frac{1-x_2}{1+x_2},$$

where
$$x_2 = \frac{2Mc\cos\phi\cos(\alpha+u)}{M^2c^2 + \cos^2\phi}$$

and
$$\tan\theta_2 = \frac{2Mc\cos\phi\sin(\alpha+u)}{M^2c^2 - \cos^2\phi}.$$

If M and α be given, these equations suffice to determine R_1, R_2, θ_1, and θ_2 completely.

For some purposes we are mainly interested in the ratio $R_1 : R_2$, and in the difference of phase $\theta_1 - \theta_2$ between the light polarised perpendicularly and parallel to the plane of incidence. Now, from the above, we have

$$\frac{R_1}{R_2} e^{i(\theta_1-\theta_2)} = \frac{r_1}{r_2} = -\frac{\tan(\phi-\phi')\sin(\phi+\phi')}{\tan(\phi+\phi')\sin(\phi-\phi')}$$

$$= -\frac{\cos(\phi+\phi')}{\cos(\phi-\phi')} = \frac{\sin\phi\sin\phi' - \cos\phi\cos\phi'}{\sin\phi\sin\phi' + \cos\phi\cos\phi'}$$

$$= \frac{\sin^2\phi - Mc\cos\phi e^{-i(\alpha+u)}}{\sin^2\phi + Mc\cos\phi e^{-i(\alpha+u)}}.$$

Thus

$$\left(\frac{R_1}{R_2}\right)^2 = \frac{M^2c^2\cos^2\phi + \sin^4\phi - 2Mc\cos\phi\sin^2\phi\cos(\alpha+u)}{M^2c^2\cos^2\phi + \sin^4\phi + 2Mc\cos\phi\sin^2\phi\cos(\alpha+u)} = \frac{1-x}{1+x},$$

where

$$x = \frac{2Mc\cos\phi\sin^2\phi\cos(\alpha+u)}{M^2c^2\cos^2\phi + \sin^4\phi},$$

and $$\tan(\theta_2-\theta_1) = \frac{2Mc\cos\phi\sin^2\phi\sin(\alpha+u)}{M^2c^2\cos^2\phi-\sin^4\phi}.$$

The last equation shows that as ϕ increases from 0 to $\frac{1}{2}\pi$, $\theta_2-\theta_1$, increases from 0 to π. We have $\theta_2-\theta_1=\frac{1}{2}\pi$, when

$$M^2c^2\cos^2\phi = \sin^4\phi,$$

and this equation accordingly determines the *Principal Incidence.*

For this angle we have

$$\frac{R_1^2}{R_2^2} = \frac{1-\cos(\alpha+u)}{1+\cos(\alpha+u)} = \tan^2\tfrac{1}{2}(\alpha+u) = \tan^2\beta,$$

where $\beta = \frac{1}{2}(\alpha+u)$ and is the *Principal Azimuth.*

These are substantially the formulæ obtained by Cauchy when discussing the problem of metallic reflection. Before putting them to the test of modern experiments, we shall make some transformations that will be useful for some purposes.

We know from the experimental investigations of Drude and others as to the optical constants of metals, that M^2 is always large. From Drude's results it is least for copper, where its value is 7·27, and greatest for zinc, where it is 34·52. This enables us to expand some of the above functions in ascending powers of $1/M^2$, and so obtain approximate formulæ sufficiently accurate for many purposes.

We have

$$ce^{-iu} = \cos\phi' = (1-M^{-2}\sin^2\phi\,.\,e^{2i\alpha})^{\frac{1}{2}} = 1-\frac{\sin^2\phi\,.\,e^{2i\alpha}}{2M^2}-\frac{\sin^4\phi\,.\,e^{4i\alpha}}{8M^4}-\frac{\sin^6\phi\,.\,e^{6i\alpha}}{16M^6}+\ldots.$$

If we multiply each side of this identity by $e^{i\alpha}$ and by $e^{-i\alpha}$, and equate real parts, we get

$$c\cos(\alpha-u) = \cos\alpha-\frac{\sin^2\phi}{2M^2}\cos3\alpha-\frac{\sin^4\phi}{8M^4}\cos5\alpha+\ldots,$$

$$c\cos(\alpha+u) = \cos\alpha+\frac{\sin^2\phi}{2M^2}\cos\alpha-\frac{\sin^4\phi}{8M^4}\cos3\alpha+\ldots.$$

Also we have

$$c^2 = \left[1-\frac{2\sin^2\phi\cos2\alpha}{M^2}+\frac{\sin^4\phi}{M^4}\right]^{\frac{1}{2}}$$
$$= 1-\frac{\sin^2\phi\cos2\alpha}{M^2}+\frac{\sin^4\phi\sin^2 2\alpha}{2M^4}+\frac{\sin^6\phi\sin2\alpha\sin4\alpha}{4M^6}+\ldots.$$

Let us now consider how R_1 varies as ϕ increases from 0° to 90°. For brevity write $M\cos\phi=p$, and we then have

$$x_1 = \frac{2pc\cos(\alpha-u)}{p^2+c^2}$$

As M^2 is large, we have, *as a first approximation*, $c = 1$ and $u = 0$, so that

$$x_1 = \frac{2p\cos\alpha}{p^2+1} = \frac{2\cos\alpha}{p+p^{-1}}.$$

This is a maximum when $p=1$, so that R_1 is then a minimum. Hence for light polarised perpendicularly to the plane of incidence the intensity of the reflected light diminishes as ϕ increases, until it reaches a minimum in the neighbourhood of $p=1$, which thus determines the "*quasi-polarising*" angle. When $p=1$ we have $x_1 = \cos\alpha$ (approximately) and $R_1 = \sqrt{(1-x_1)/(1+x_1)} = \tan\frac{1}{2}\alpha$. For a certain class of steel we shall find later that $M^2=13$, and $\alpha=53^\circ\,42'$. In this case $p=1$, or $\cos\phi = M^{-1}$, gives $\phi = 73^\circ\,54'$ as the quasi-polarising angle, and $R_1 = 0{\cdot}5062$ as the minimum value of the amplitude of the reflected wave. This approximation is, of course, somewhat rough, as we have neglected squares and higher powers of $1/M^2$. Proceeding to a higher order, we get

$$x_1 = \frac{2p\left(\cos\alpha - \frac{1}{2}M^{-2}\sin^2\phi\cos 3\alpha - \frac{1}{8}M^{-4}\sin^4\phi\cos 5\alpha\right)}{p^2+1-M^{-2}\sin^2\phi\cos 2\alpha + \frac{1}{2}M^{-4}\sin^4\phi\sin^2 2\alpha}.$$

In the small terms we may put the results of the first approximation $p=1$ and $\sin^2\phi = 1-M^{-2}$. This gives

$$x_1 = \frac{2p\left(\cos\alpha - \frac{1}{2}M^{-2}\cos 3\alpha + \frac{1}{2}M^{-4}\cos 3\alpha - \frac{1}{8}M^{-4}\cos 5\alpha\right)}{p^2+1-M^{-2}\cos 2\alpha + M^{-4}\cos 2\alpha + \frac{1}{2}M^{-4}\sin^2 2\alpha}.$$

This makes R_1 a minimum when $p^2 = 1-M^{-2}\cos 2\alpha + M^{-4}\cos 2\alpha + \frac{1}{2}M^{-4}\sin^2 2\alpha$. With the values of M and α given above for steel, this fixes the quasi-polarising angle at $\phi = 73^\circ\,43'$.

In light polarised parallel to the plane of incidence we have

$$x_2 = \frac{2Mc\cos\phi\cos(\alpha+u)}{M^2c^2+\cos^2\phi}.$$

As ϕ increases the denominator alters little, as $\cos^2\phi$ is always small compared with M^2c^2, while the numerator steadily decreases. Thus R_2 increases steadily, and as the equation to determine its maxima or minima has no real roots, it has no maximum or minimum.

We have seen that the Principal Incidence is determined by the equation $\sin^4\phi = M^2c^2\cos^2\phi$. Putting

$$c = 1-\tfrac{1}{2}M^{-2}\sin^2\phi\cos 2\alpha + \tfrac{1}{8}M^{-4}\sin^4\phi\,(2\sin^2 2\alpha - \cos^2 2\alpha),$$

we get as the approximate equation to determine the Principal Incidence,

$$\sec\phi = M - \tfrac{1}{2}M^{-1}\sin^2\phi\cos 2\alpha + \tfrac{1}{8}M^{-3}\sin^4\phi\,(2\sin^2 2\alpha - \cos^2 2\alpha)$$
$$+ M^{-1}(1+\tfrac{1}{2}M^{-2}\sin^2\phi\cos 2\alpha).$$

This is most simply solved by approximations. The first approximation gives $\sec\phi = M$, so that $p = M\cos\phi = 1$. To this order of approximation

the Principal Incidence and the quasi-polarising angle are the same, so that as a rule the Principal Incidence will be very near the quasi-polarising angle.

The second approximation gives

$$\sec\phi = M + M^{-1}(1 - \tfrac{1}{2}\cos 2\alpha),$$

and the third,

$$\sec\phi = M + M^{-1}(1 - \tfrac{1}{2}\cos 2\alpha) + M^{-3}\{\cos 2\alpha + \tfrac{1}{16}(1 - 3\cos 4\alpha)\}.$$

If $M = 4$, an error of $1/40$ in $\sec\phi$ corresponds to an error of about five minutes in ϕ, and this is within the limits of experimental error. In this case $1/M^3 = 1/64$, so that the second approximation will give the Principal Incidence sufficiently accurately for comparison with experimental results.

Returning now to the exact formulæ with which we began, we shall see how they fit in with modern experiments on metallic reflection. It has been usual* to compare them with the results of Jamin's experiments on steel and other metals. When this is done it is found that the theory fits in with the experiment as far as the main features of the metallic reflection are concerned; but it is only necessary to plot the experimental results to see that they are much too discordant to afford a satisfactory test.

Since Jamin's time there have been many experimental investigations into the phenomena of metallic reflection. Amongst others, we have an elaborate series of experiments by Sir John Conroy† on reflection from steel and speculum metal. Conroy found for steel that the Principal Incidence was 76° 20′ (with a probable error of $\pm 5'$) and the Principal Azimuth 28° 29′ (with a probable error of $\pm 1'$). He made four separate sets of experiments for reflection of light polarised perpendicularly and parallel to the plane of incidence. By rejecting the most discordant of the four when there is considerable discordance, and taking the mean of the remaining ones, and by taking the mean of the four where, as is generally the case, there is no great discordance, we get the following table, the notation being the same as at the outset of this paper:—

ϕ.	R_1.	R_2.	ϕ.	R_1.	R_2.
30°	0·7084	0·7791	70°	0·5152	0·9069
40	0·6803	0·8013	75	0·5047	0·9275
50	0·6401	0·8331	80	0·5254	0·9501
60	0·5837	0·8627			

* See, *e.g.*, Mascart's 'Traité d'Optique,' t. 2, vol. 13.

† See 'Roy. Soc. Proc.,' vol. 36, p. 187.

On plotting these numbers they will be found to be very fairly self-consistent, and to agree well with the observed value of the Principal Azimuth, the only exception being that R_1 appears to be too small at 80°.

There are two unknown constants, M and α, and these can be determined from a knowledge of the Principal Azimuth and Principal Incidence. At the Principal Incidence we have

$$Mc = \sin^2\phi/\cos\phi \quad \text{and} \quad \alpha+u = 2\beta.$$

Also

$$c^2 \sin 2u = M^{-2}\sin^2\phi \sin 2\alpha.$$

Thus

$$\sin 2u \tan^2\phi = \sin 2\alpha = \sin(4\beta-2u),$$

so that

$$\tan 2u = \frac{\sin 4\beta}{\tan^2\phi+\cos 4\beta}.$$

Taking $\phi = 76° \, 20'$ and $\beta = 28° \, 29'$, this gives $u = 1° \, 35'$ and $\alpha = 55° \, 23'$.

Also we have

$$\cot 2u = \frac{M^2-\sin^2\phi\cos 2\alpha}{\sin^2\phi \sin 2\alpha};$$

thus

$$M^2 = \frac{\sin^2\phi \sin 4\beta}{\sin 2u} = 15{\cdot}67.$$

This gives $\mu = M\cos\alpha = 2{\cdot}249$ and $a = M\sin\alpha = 3{\cdot}257$.

Having obtained α and M, we can calculate c and u for any value of ϕ from the formulæ

$$\cot 2u = \frac{M^2}{\sin^2\phi\sin 2\alpha} - \cot 2\alpha \quad \text{and} \quad c^2 = \frac{\sin^2\phi\sin 2\alpha}{M^2\sin 2u}.$$

Owing, however, to the smallness of u, the latter formula is not a very good one from which to determine c^2, since, particularly when ϕ is not large, the variations of cosec $2u$ are very rapid, so that a small error in u will affect c^2 considerably. We can avoid this difficulty by using the formula

$$c^4 = 1-2M^{-2}\sin^2\phi\cos 2\alpha + M^{-4}\sin^4\phi.$$

We thus get the following table:—

ϕ.	u.	c.	R_1 (theory).	Diff. from experiment.	R_2 (theory).	Diff. from experiment.
30°	0° 26′	1·003	0·7264	+0·0180	0·7879	+0·0088
40	0 42	1·005	0·6979	+0·0176	0·8106	+0·0093
50	0 59	1·006	0·6564	+0·0163	0·8389	+0·0058
60	1 16	1·009	0·5969	+0·0132	0·8728	+0·0101
70	1 29	1·012	0·5271	+0·0119	0·9111	+0·0042
75	1 34	1·012	0·5077	+0·0030	0·9822	+0·0047
80	1 37	1·014	0·5398	+0·0144	0·9541	+0·0040

We see from this table, and also from the graphical representation of these results in the figure below, that there is a considerable discrepancy between

Fig. 1.

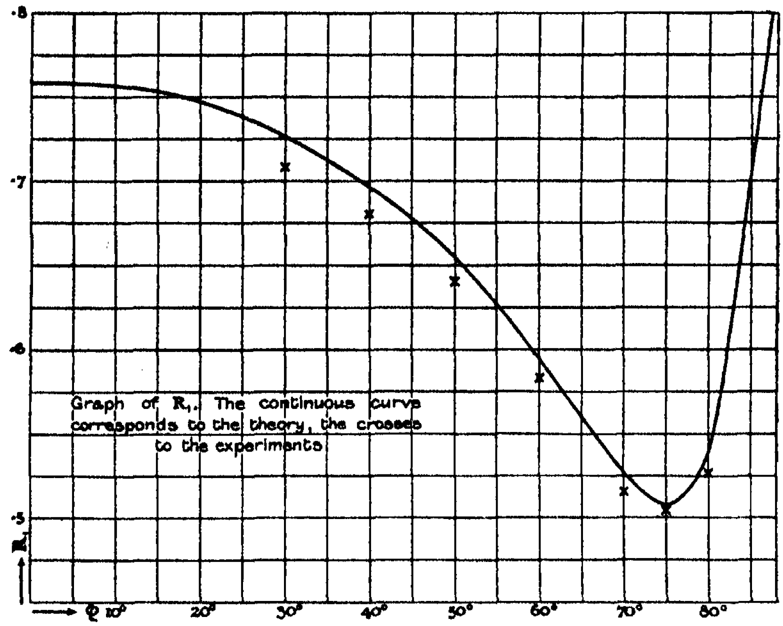
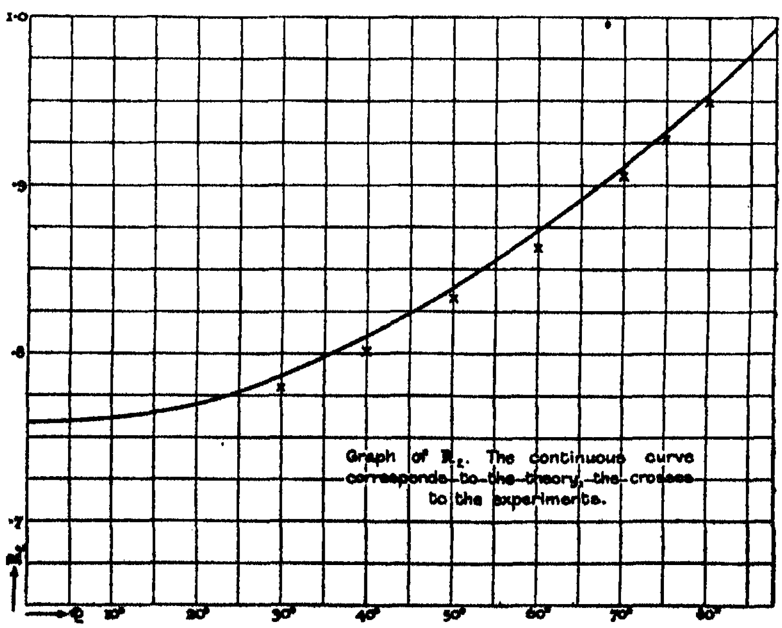


Fig. 2.



theory and experiment. The values of R_1 are almost uniformly between 1 and 2 per cent. larger than those found by experiment, while the values of R_2 are larger by quantities varying from between $\frac{1}{2}$ and 1 per cent. The differences are rather too large to be put down to experiment, and the fact that they are all of the same sign makes it improbable that they represent experimental errors. Moreover, on making a similar calculation for speculum metal, we find that the values are in this case always larger than those found by Conroy, the difference being greatest for R_2, where it amounts to $2\frac{1}{2}$ per cent. in some cases.

When dealing with transparent media it has been found that the discrepancies between theory and experiment have disappeared as soon as it has been recognised that the transition from one medium to another is, as a rule, gradual and not abrupt. In the present paper we shall investigate the extent to which this idea will assist us when dealing with the problem of metallic reflection.

When the layer of transition is taken into account, the formulæ for r_1 and r_2 take the forms*

$$r_1 = \frac{\kappa_0\mu_1^2/\mu_0^2 - \kappa_1 + id_1(F\nu^2 - \mu_1^2 + E\kappa_0\kappa_1\mu_1^2/\mu_0^2)}{\kappa_0\mu_1^2/\mu_0^2 + \kappa_1 - id_1(F\nu^2 - \mu_1^2 - E\kappa_0\kappa_1\mu_1^2/\mu_0^2)}$$

and
$$r_2 = \frac{\kappa_0 - \kappa_1 + id_1(\kappa_0\kappa_1 - E\mu_1^2 + \nu^2)}{\kappa_0 + \kappa_1 + id_1(\kappa_0\kappa_1 + E\mu_1^2 - \nu^2)}.$$

In our present notation $\nu = \sin\phi$, $\kappa_0 = \cos\phi$, $\kappa_1 = \mu\cos\phi'$, $\mu_0 = 1$, $\mu_1 = \mu$; so that we have

$$r_1 = \frac{\mu\cos\phi - \cos\phi' + id_1(\mu^{-1}F\sin^2\phi - \mu + E\mu^2\cos\phi\cos\phi')}{\mu\cos\phi + \cos\phi' - id_1(\mu^{-1}F\sin^2\phi - \mu - E\mu^2\cos\phi\cos\phi')}$$

and
$$r_2 = \frac{\cos\phi - \mu\cos\phi' + id_1(\mu\cos\phi\cos\phi' - E\mu^2 + \sin^2\phi)}{\cos\phi + \mu\cos\phi' + id_1(\mu\cos\phi\cos\phi' + E\mu^2 - \sin^2\phi)}.$$

Here $d_1 = 2\pi d/\lambda$, where d is the thickness of the layer, and λ the wave-length in air. E and F are complex constants defined by the equations $E = \int_0^1 \mu^2 dx$ and $F = \mu^2\int_0^1 \mu^{-2}dx$. The values of these constants depend, of course, on the law of variation of μ in the layer of transition. This being unknown, we cannot determine E and F, but we should expect them to lie between 1 and μ^2. If μ^2 had the value $\frac{1}{2}(1+\mu^2)$ in the layer, we should have $E = \frac{1}{2}(1+\mu^2)$ and $F = 2\mu^2/(1+\mu^2)$, so that if (as with steel) the modulus of μ^2 were about 13, $|E|$ would be about 7 and $|F|$ about 2. But in any case, owing to the largeness of μ^2, the term $E\mu^2\cos\phi\cos\phi'$ in the formula for r_1 will be large compared with $\mu^{-1}F\sin^2\phi - \mu$, except where ϕ is

* See a paper by the present writer, 'Roy. Soc. Proc.,' A, vol. 76, pp. 55 and 57.

very nearly 90° when the term introduced by the layer of transition becomes $\mu^{-1}F-\mu$, which is small compared with $E\mu^2$. The sequel will prove that d_1 is very small, so that the changes due to the layer will be small.

Putting $id_1E\mu^2 = ae^{iw}$, neglecting the term $\mu^{-1}F\sin^2\phi-\mu$ in comparison with $E\mu^2\cos\phi\cos\phi'$, and putting $\cos\phi' = 1$ in the *small* terms, we get:—

$$r_1 = \frac{\mu\cos\phi-\cos\phi'+a\cos\phi\,.\,e^{iw}}{\mu\cos\phi+\cos\phi'+a\cos\phi\,.\,e^{iw}} = \frac{A_1e^{iv_1}+a\cos\phi\,.\,e^{iw}}{A_1'e^{iv_1'}+a\cos\phi\,.\,e^{iw}},$$

where $a\cos\phi$ is small compared with A_1 and A_1'.

If the modulus of r_1 be $R_1+\rho_1$, where R_1 is obtained from the formula of p. 212, then ρ_1 is the *correction* to R_1 due to the layer of transition.

We have

$$(R_1+\rho_1)^2 = \frac{A_1^2+a^2\cos^2\phi+2A_1\,a\cos\phi\cos(v_1-w)}{A_1'^2+a^2\cos^2\phi+2A_1'\,a\cos\phi\cos(v_1'-w)},$$

whence we have, approximately,

$$\rho_1 = R_1\,a\cos\phi\left[\frac{\cos(v_1-w)}{A_1}-\frac{\cos(v_1'-w)}{A_1'}\right].$$

Now
$$A_1e^{iv_1} = \mu\cos\phi-\cos\phi' = Me^{-i\alpha}\cos\phi-ce^{-iu},$$
$$A_1'e^{iv_1'} = \mu\cos\phi+\cos\phi' = Me^{-i\alpha}\cos\phi+ce^{-iu},$$

whence, employing the approximate values $c = 1$, $u = 0$, we have

$$A_1^2 = M^2\cos^2\phi+c^2-2M\cos\phi\cos\alpha,$$
$$A_1'^2 = M^2\cos^2\phi+1+2M\cos\phi\cos\alpha.$$

$$\tan v_1 = \frac{-M\cos\phi\sin\alpha}{M\cos\alpha\cos\phi-1}; \qquad \tan v_1' = -\frac{M\cos\phi\sin\alpha}{M\cos\alpha\cos\phi+1}.$$

$$\cos v_1 = \frac{1-M\cos\alpha\cos\phi}{A_1}; \qquad \cos v_1' = -\frac{1+M\cos\alpha\cos\phi}{A_1'},$$

$$\sin v_1 = \frac{M\cos\phi\sin\alpha}{A_1}; \qquad \sin v_1' = \frac{M\cos\phi\sin\alpha}{A_1'}.$$

Making these substitutions in the formula for ρ, we get

$$\begin{aligned}\rho_1 &= R_1\,a\cos\phi\left[\cos w\left\{\frac{\cos v_1}{A_1}-\frac{\cos v_1'}{A_1'}\right\}+\sin w\left\{\frac{\sin v_1}{A_1}-\frac{\sin v_1}{A_1'}\right\}\right]\\ &= R_1\,a\cos\phi\left[\cos w\left\{\frac{1-M\cos\alpha\cos\phi}{A_1^2}+\frac{1+M\cos\alpha\cos\phi}{A_1'^2}\right\}\right.\\ &\qquad\left.+M\sin\alpha\cos\phi\sin w\left\{\frac{1}{A_1^2}-\frac{1}{A_1'^2}\right\}\right]\\ &= \frac{2R_1\,a\cos\phi}{A_1^2A_1'^2}\left[\cos\psi-M^2\cos^2\phi\cos(2\alpha+w)\right].\end{aligned}$$

This makes ρ_1 vanish when $\phi = 90°$ and also when

$$\cos\phi = M^{-1}\sqrt{\cos w/\cos(2\alpha+w)}.$$

In considering the law of variation of ρ, it is convenient to ascertain the position of its maxima and minima. Putting $M\cos\phi = p$ we have ρ_1 proportional to $R_1 p\,(p^2-b)/A_1^2 A_1'^2$, where $b = \cos w/\cos(2\alpha+w)$.

In the small terms we may use the approximate formulæ $c = 1$ and $v = 0$. We then have

$$A_1^2 A_1'^2 = (1+p^2)^2 - 4p^2\cos^2\alpha = 1 - 2p^2\cos 2\alpha + p^4,$$

and
$$R_1 = \sqrt{\frac{1-x_1}{1+x_1}}, \quad \text{where } x_1 = \frac{2p\cos\alpha}{1+p^2}.$$

Thus
$$-\frac{1}{R_1}\frac{dR_1}{dp} = \frac{1}{1-x_1^2}\frac{dx_1}{dp} = \frac{2\cos\alpha\,(1-p^2)}{1-2p^2\cos 2\alpha+p^4}.$$

The equation to determine the maxima and minima is, then,

$$\frac{3p^2-b}{p\,(q^2-b)} = \frac{2\cos\alpha\,(1-p^2)+4p\,(p^2-\cos 2\alpha)}{1-2p^2\cos 2\alpha+p^4},$$

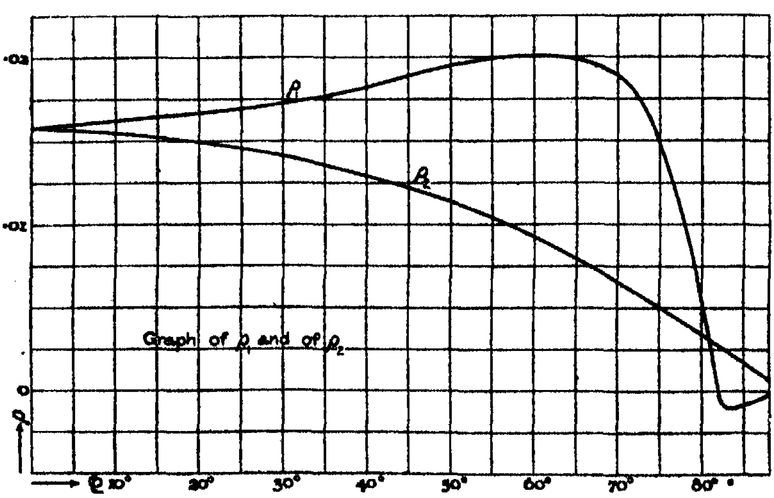
i.e., $$p^6 - 2\cos\alpha\,.\,p^5 + (2\cos 2\alpha - 3b)\,p^4 + 2\,(1+b)\cos\alpha\,.\,p^3 + (2b\cos 2\alpha - 3)\,p^2 - 2b\cos\alpha\,.\,p + b = 0.$$

With the values of α and w found later for steel this equation becomes

$$p^6 - 1{\cdot}176p^5 - 0{\cdot}8178p^4 + 1{\cdot}254p^3 - 3{\cdot}041p^2 - 0{\cdot}0783p + 0{\cdot}0666 = 0.$$

Solving this, by Horner's process, we get two real roots, $p = 0{\cdot}37$ and $p = 1{\cdot}8$. The former corresponds to $\phi = 84°\ 6'$ and determines the position of the minimum, the latter corresponds to $\phi = 60°$ and determines the position of the maximum. The graph below (fig. 3) represents the march of the function ρ_1 in the case of steel.

FIG. 3.



In dealing similarly with r_2 we note that the modulus of $E\mu^2$ is large compared with that of $\mu\cos\phi\cos\phi'+\sin^2\phi$. Neglecting the latter term in comparison with the former we get

$$r_2 = \frac{\cos\phi-\mu\cos\phi'-id_1 E\mu^2}{\cos\phi+\mu\cos\phi'+id_1 E\mu^2} = \frac{A_2 e^{iv_2}-ae^{iw}}{A_2' e^{iv_2'}+ae^{iw}}.$$

If the modulus of r_2 be $R_2+\rho_2$, so that ρ_2 is the *correction* due to the layer of transition, we have

$$\rho_2 = -R_2 a\left[\frac{\cos(v_2-w)}{A_2}+\frac{\cos(v_2'-w)}{A_2'}\right],$$

$$A_2 e^{iv_2} = \cos\phi-\mu\cos\phi'; \qquad A_2' e^{iv_2'} = \cos\phi+\mu\cos\phi',$$

whence, approximately, $A_2^2 = M^2+\cos^2\phi-2M\cos\phi\cos\alpha,$

$$A_2'^2 = M^2+\cos^2\phi+2M\cos\phi\cos\alpha;$$

$$\tan v_2 = \frac{M\sin\alpha}{\cos\phi-M\cos\alpha}; \qquad \tan v_2' = \frac{-M\sin\alpha}{\cos\phi+M\cos\alpha};$$

so that
$$\rho_2 = \frac{2R_2 a\cos\phi}{A_2 A_2'^2}[\cos^2\phi\cos w-M^2\cos(2\alpha+w)],$$

and ρ_2 will thus vanish when $\phi = 90°$. It would also vanish if $\cos^2\phi = M^2\cos(2\alpha+w)/\cos w$; but, as a rule, this will be larger than unity, and so there will be no real value of ϕ to satisfy it.

In the formula for ρ_2 the term $\cos^2\phi\cos w$ will usually be negligible in comparison with $M^2\cos(2\alpha+w)$, so that we shall have, approximately,

$$\rho_2 = -2R_2 a\cos(2\alpha+w)\cos\phi/A_2^2 A_2'^2.$$

Now
$$A_2^2 A_2'^2 = (M^2+\cos^2\phi)^2-4M^2\cos^2\phi\cos^2\alpha$$
$$= \cos^4\phi+M^4+2M^2\cos^2\phi\,(1-2\cos^2\alpha).$$

For almost all the metals α is greater than 45°, so that $1-2\cos^2\alpha$ is positive. Thus $A_2^2 A_2'^2$ is greater than M^4 and ρ_2 is less than

$$2M^{-2}R_2\, a\cos\phi\cos(2\alpha+w).$$

Owing to the factor M^{-2}, ρ_2 will thus be small and it will diminish steadily with ϕ. If we investigate the position of the maxima and minima in the same manner as we did with ρ_1 we are led to the following equation :—

$$\left(\frac{4}{M^6}-\frac{3}{M^8}\right)p^6-\frac{2\cos\alpha}{M^6}p^5+\left(\frac{6\cos 2\alpha}{M^4}-\frac{4b}{M^6}+\frac{b}{M^8}\right)p^4$$
$$+\left(\frac{2\cos\alpha}{M^2}-\frac{4\cos 2\alpha}{M^4}+\frac{2b\cos\alpha}{M^6}\right)p^3-\left(3-\frac{2b\cos 2\alpha}{M^4}\right)p^2-\frac{2b\cos\alpha}{M^2}p-6 = 0.$$

As $p = M\cos\phi$, p cannot be greater than M, and the above equation in p has no real roots less than M, so that there are no maxima and minima.

The graph above (fig. 3) represents the march of the function ρ_2 in the case of steel. It will be seen from this figure that the range for which ρ_1 and ρ_2 are appreciable is much larger than in the case of a transparent medium, where the influence of the transition layer is practically confined within a few degrees of the polarising angle.

It has been observed before that for many purposes the *ratio* of R_1 to R_2, and the *difference* of phase between light polarised perpendicularly and parallel to the plane of incidence is what is wanted for comparison with experiment. We proceed to develop some formulæ suitable for this end.

We have

$$r_1 = \frac{\mu\cos\phi - \cos\phi' + id_1 E\mu^2\cos\phi\cos\phi'}{\mu\cos\phi + \cos\phi' + id_1 E\mu^2\cos\phi\cos\phi'}$$

$$= \frac{\sin(\phi-\phi')\cos(\phi+\phi') + a\sin\phi'\cos\phi'\cos\phi e^{iw}}{\sin(\phi+\phi')\cos(\phi-\phi') + a\sin\phi'\cos\phi'\cos\phi e^{iw}};$$

$$r_2 = \frac{\cos\phi - \mu\cos\phi' - id_1 E\mu^2}{\cos\phi + \mu\sin\phi' + id_1 E\mu^2} = -\frac{\sin(\phi-\phi') + a\sin\phi' e^{iw}}{\sin(\phi+\phi') + a\sin\phi' e^{iw}}.$$

Thus

$$\frac{r_1}{r_2} = -\frac{\sin(\phi-\phi')\cos(\phi+\phi') + a\sin\phi'\cos\phi'\cos\phi e^{iw}}{\sin(\phi+\phi')\cos(\phi-\phi') + a\sin\phi'\cos\phi'\cos\phi e^{iw}} \times \frac{\sin(\phi+\phi') + a\sin\phi' e^{iw}}{\sin(\phi-\phi') + a\sin\phi' e^{iw}}$$

$$= -\frac{A+a'}{A'+a'}\cdot\frac{B+b'}{B'+b'},$$ where the moduli of a' and b' are small compared with those of A and B,

$$= -\frac{AB}{A'B'}\left[1 + a'\left(\frac{1}{A} - \frac{1}{A'}\right) + b'\left(\frac{1}{B} - \frac{1}{B'}\right)\right]$$

$$= -\frac{\cos(\phi+\phi')}{\cos(\phi-\phi')}\left[1 + a\sin\phi'\cos\phi'\cos\phi e^{iw}\left\{\frac{1}{\sin(\phi-\phi')\cos(\phi+\phi')} - \frac{1}{\sin(\phi+\phi')\cos(\phi-\phi')}\right\} + a\sin\phi' e^{iw}\left\{\frac{1}{\sin(\phi+\phi')} - \frac{1}{\sin(\phi-\phi')}\right\}\right]$$

$$= -\frac{\cos(\phi+\phi')}{\cos(\phi-\phi')}\left[1 + \frac{2a\sin^2\phi'\cos\phi . e^{iw}\{\cos^2\phi' - \cos(\phi-\phi')\cos(\phi+\phi')\}}{\sin(\phi+\phi')\cos(\phi+\phi')\sin(\phi-\phi')\cos(\phi-\phi')}\right]$$

$$= -\frac{\cos(\phi+\phi')}{\cos(\phi-\phi')}\left[1 - \frac{2a\sin^2\phi'\sin^2\phi\cos\phi\, e^{iw}}{\sin(\phi+\phi')\cos(\phi+\phi')\sin(\phi-\phi')\cos(\phi-\phi')}\right]$$

$$= R_1/R_2 . e^{i(\theta_1-\theta_2)}[1 - ye^{i\theta}],$$

where
$$ye^{i\theta} = \frac{2a\sin^2\phi'\sin^2\phi\cos\phi e^{iw}}{\sin(\phi+\phi')\cos(\phi+\phi')\sin(\phi-\phi')\cos(\phi-\phi')}.$$

Now

$$\sin(\phi+\phi')\cos(\phi+\phi')\sin(\phi-\phi')\cos(\phi-\phi')$$
$$= \tfrac{1}{2}(\sin 2\phi + \sin 2\phi') \times \tfrac{1}{2}(\sin 2\phi - \sin 2\phi')$$
$$= \tfrac{1}{4}(\sin^2 2\phi - \sin^2 2\phi') = \sin^2\phi\cos^2\phi - \sin^2\phi'\cos^2\phi', \;\; . \;\; .$$

and remembering that

$$\cos\phi' = ce^{-iu} \quad \text{and} \quad \sin\phi' = \mathrm{M}^{-1}e^{i\alpha}\sin\phi,$$

we get
$$qe^{i\theta} = \frac{2a\sin^2\phi\cos\phi \, . \, e^{i(2\alpha+w)}}{\mathrm{M}^2\cos^2\phi - c^2e^{2i(\alpha-u)}}.$$

Hence

$$q = \frac{2a\sin^2\phi\cos\phi}{[\mathrm{M}^4\cos^4\phi + c^4 - 2\mathrm{M}^2c^2\cos^2\phi\cos 2(\alpha-u)]^{\frac{1}{2}}}$$

$$= 2a\sin^2\phi\cos\phi/\mathrm{A}_1\mathrm{A}_1', \text{ in the notation of p. 219,}$$

$$\theta = 2\alpha + w + \theta', \quad \text{where} \quad \tan\theta' = \frac{c^2\sin 2(\alpha-u)}{\mathrm{M}^2\cos^2\phi - c^2\cos 2(\alpha-u)}.$$

To determine the maximum value of q we put $\mathrm{M}\cos\phi = p$, as before, and using the approximate relations $c = 1$ and $u = 0$, we have to make $p(1-p^2/\mathrm{M}^2)/\sqrt{p^4-2p^2\cos 2\alpha+1}$ a maximum.

This requires

$$(p^4-2p^2\cos 2\alpha+1)(1-3p^2\mathrm{M}^{-2}) - 2p^2(1-p^2\mathrm{M}^{-2})(p^2-\cos 2\alpha) = 0.$$

Solving this by approximations we get $p = 1$ as the first approximation. This, as we have seen (p. 214), is the first approximation to the quasi-polarising angle. A second approximation gives $p^4 = 1 - 8\mathrm{M}^{-2}\sin^2\alpha$. If $\mathrm{M}^2 = 13$ and $\alpha = 53° \, 42'$, as we shall find later for a certain class of steel, the first approximation gives $\phi = 73° \, 54'$, and the second $\phi = 75° \, 52'$ which is very near the Principal Incidence.

If we put $\quad 1 - qe^{i\theta} = se^{i\chi}$, we have $\dfrac{\mathrm{R}_1+\rho_1}{\mathrm{R}_2+\rho_2} = s\dfrac{\mathrm{R}_1}{\mathrm{R}_2}$,

and the difference of phase is $\theta_1 - \theta_2 + \chi$.

Here $\quad s = \sqrt{1-2q\cos\theta+q^2} \quad$ and $\quad \tan\chi = \dfrac{-q\sin\theta}{1-q\cos\theta}$.

If, as will usually be the case, q be small, χ will be small and the correction to the change of phase will be small. It will be greatest when q is largest, *i.e.*, in the neighbourhood of the Principal Incidence.

Perhaps, however, the most important point to notice is that, even although the correction to the change of phase be small, it may make an appreciable difference to the position of the Principal Incidence. At the Principal Incidence we have

$$\theta_1 - \theta_2 + \chi = \tfrac{1}{2}\pi, \quad \text{therefore} \quad \cot(\theta_1-\theta_2) = \tan\chi.$$

or
$$\sin^4\phi - \mathrm{M}^2c^2\cos^2\phi = 2\mathrm{M}c\cos\phi\sin^2\phi\sin(\alpha+u)\tan\chi.$$

This is the equation to determine the Principal Incidence, and owing to the presence of M on the right hand side, that term may be appreciable even although χ be small.

It would seem then, that two constants (M and α) are not sufficient to describe the optical properties of a metal—at least two more (a and w) are required in the problem of reflection, these latter constants depending on the law of variation of μ^2 in the layer of transition between the media. This being the case, we cannot derive the optical constants from observations of the Principal Azimuth and Principal Incidence alone. The simplest method is to proceed by successive approximations. The true values of M and α will be smaller than those obtained by neglecting the layer of transition and proceeding as on p. 216. On obtaining approximate values of M and α in this way, we can calculate the constants a and w from observation of the Principal Azimuth (β) and Principal Incidence (ϕ).

We have

$$\sqrt{1-2q\cos\theta+q^2} = \frac{R_1+\rho_1}{R_2+\rho_2}\Big/\frac{R_1}{R_2} = \frac{R_2}{R_1}\tan\beta, \tag{i}$$

and

$$\frac{-q\sin\theta}{1-q\cos\theta} = \tan\chi = \frac{\sin^4\phi - Mc^2\cos^2\phi}{2\,Mc\cos\phi\sin^2\phi\sin^2(\alpha+u)} \tag{ii}$$

These are two equations, from which q and θ may be readily determined.

Equation (ii) determines χ, and we then have

$$\tan\theta = \frac{R_2/R_1\,.\tan\beta\sin\chi}{R_2/R_1\,.\tan\beta\cos\chi-1}; \qquad q = -R_2/R_1\,.\tan\beta\sin\chi\operatorname{cosec}\theta,$$

and once q is determined we derive a from the equation

$$q = 2a\sin^2\phi\cos\phi/A_1A_1'.$$

We shall apply this method to Conroy's experiments on reflection from steel. The values of M^2 and α already obtained (p. 216) are too high. We shall take as the next approximation $M^2 = 14{\cdot}44$ and $\alpha = 54°$, although the sequel will prove that these are still too high. Taking $\beta = 28°\ 29'$ and $\phi = 76°\ 20'$ from Conroy's experiments, we get the following from the equations just obtained :—

$$\chi = 2°\ 42'; \qquad \theta = 180° + 64°\ 32'$$
$$q = 0{\cdot}0535; \qquad a = 0{\cdot}1742;$$
$$\theta' = 42°\ 59'; \qquad w = 93°\ 33'.$$

With $M^2 = 14{\cdot}44$ and $\alpha = 54°$ the formulæ of p. 212 give us the following table :—

ϕ.	a.	c.	R_1.	R_2.
0°	0° 0′	1	0·7423	0·7423
30	0 28	1·007	0·7074	0·7745
40	0 46	1·007	0·6790	0·7979
50	1 6	1·007	0·6352	0·8279
60	1 23	1·009	0·5751	0·8636
70	1 38	1·010	0·5063	0·9046
75	1 43	1·010	0·4911	0·9268
80	1 47	1·011	0·5304	0·9504

The formulæ for ρ_1 and ρ_2 on pp. 219 and 221, with the above values of a, R_1, and R_2, give us the the following:—

ϕ.	ρ_1.	ρ_2.	ϕ.	ρ_1.	ρ_2.
0°	0·0158	0·0158	60°	0·0206	0·0092
30	0·0172	0·0141	70	0·0191	0·0066
40	0·0182	0·0129	75	0·0150	0·0051
50	0·0195	0·0114	80	0·0060	0·0035

These results are represented graphically in fig. 3 above.

If we take these values of R_1, R_2, ρ_1, and ρ_2, and compare $R_1 + \rho_1$ and $R_2 + \rho_2$ with the results of Conroy's experiments as set out on p. 215, we shall find that R_1 and R_2 are still too large to fit in with the experimental data. This indicates that we must further depress M and α; but now that the correction ρ_1 and ρ_2 have been approximately estimated, there is not the same guesswork in seeking for the correct values of M and α. As a second approximation we shall take $M^2 = 13$ and $\alpha = 53° 42'$. With these we get:—

ϕ.	R_1.	R_2.	ϕ.	R_1.	R_2.
0°	0·7300	0·7300	60°	0·5611	0·8557
30	0·6942	0·7630	70	0·4978	0·8991
40	0·6639	0·7874	75	0·4893	0·9230
50	0·6204	0·8183	80	0·5368	0·9473

The change of M and α will, of course, affect all the other quantities including the corrections ρ_1 and ρ_2. The alterations will not amount, however, to more than 1 per cent., and such a fraction of ρ_1 and ρ_2 is scarcely

worth considering in view of the uncertainty of the experimental results. We shall, therefore, take ρ_1 and ρ_2 to have the same values as those calculated for $M^2 = 14{\cdot}44$ and $\alpha = 54°$. This will give us the following table, in which the results are compared with Conroy's numbers set out on p. 215.

ϕ.	$R_1 + \rho_1$.		Difference.
	Theory.	Experiment.	
30°	0·7114	0·7084	+0·0030
40	0·6821	0·6803	+0·0018
50	0·6399	0·6401	−0·0002
60	0·5817	0·5837	−0·0020
70	0·5169	0·5152	+0·0017
75	0·5043	0·5047	−0·0004
80	0·5428	0·5254	+0·0174

ϕ.	$R_2 + \rho_2$.		Difference.
	Theory.	Experiment.	
30°	0·7771	0·7791	−0·0020
40	0·8003	0·8013	−0·0010
50	0·8297	0·8331	−0·0034
60	0·8649	0·8627	+0·0022
70	0·9057	0·9069	−0·0012
75	0·9281	0·9275	+0·0006
80	0·9508	0·9501	+0·0007

It will be seen that the differences between theory and experiment are well within the limits of experimental error. The only appreciable difference is for R_1 at 80°, and we have already noted (p. 218) that there is reason to doubt the accuracy of the experiments in this case. The results are exhibited graphically in figs. 4 and 5.

The difference of phase between the light polarised parallel, and that polarised perpendicularly to the plane of incidence is $\theta_2 - \theta_1 - \chi$, where

$$\tan(\theta_2 - \theta_1) = \frac{2Mc\cos\phi\sin^2\phi\sin(\alpha+u)}{M^2c^2\cos^2\phi - \sin^4\phi};$$

$$\tan\chi = \frac{-q\sin\theta}{1-q\cos\theta}; \qquad q = \frac{2a\sin^2\phi\cos\phi}{A_1A_1'};$$

$$\theta = 2\alpha + w + \theta'; \qquad \tan\theta' = \frac{c^2\sin 2(\alpha-u)}{M^2\cos^2\phi - c^2\cos 2(\alpha-u)}.$$

FIG. 4.

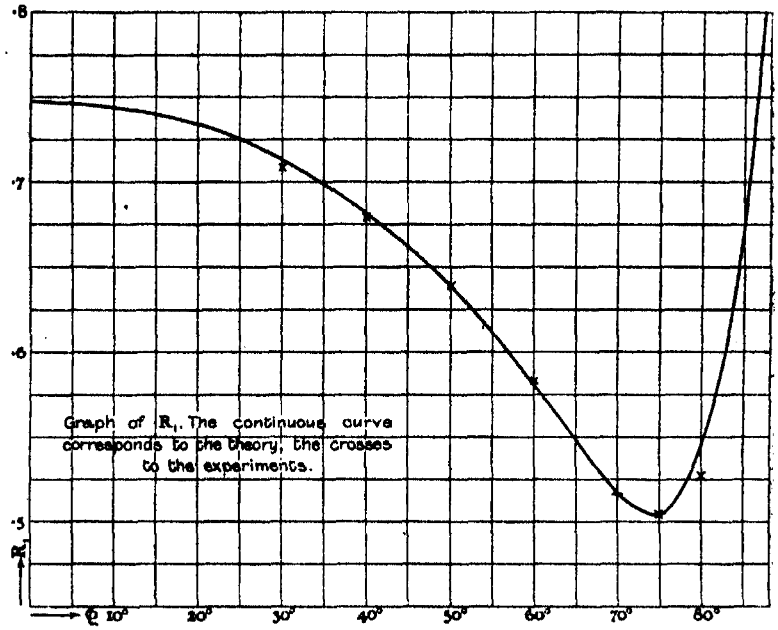
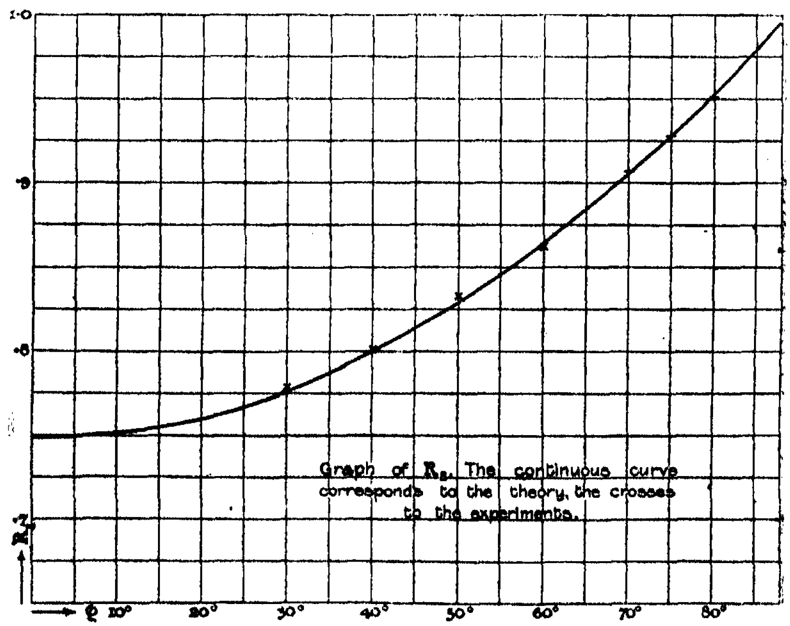

·8
·7
·6
·5
R_1
Graph of R_1. The continuous curve corresponds to the theory, the crosses to the experiments.
ϱ 10°
20°
30°
40°
50°
60°
70°
80°

FIG. 5.


1·0
·9
·8
·7
R_2
Graph of R_2. The continuous curve corresponds to the theory, the crosses to the experiments.
ϱ 10°
20°
30°
40°
50°
60°
70°
80°

From these, with the numerical values of the constants adopted above, we derive the following:—

ϕ.	θ'.	$\theta'-180°$.	ϕ.	θ'.	$\theta'-180°$.
30°	5° 32′	26° 29′	70°	29° 13′	50° 10′
40	7 2	27 59	75	41 35	62 32
50	9 51	30 48	80	57 22	78 19
60	15 40	36 37			

While for q and χ we get :—

ϕ.	q.	χ.	ϕ.	q.	χ.
0°	0	0° 0′	60°	0·0327	1° 5′
30	0·0068	0 10	70	0·0482	2 3
40	0·0125	0 20	75	0·0585	2 39
50	0·0208	0 36	80	0·0486	2 42

From these we derive the following table for the difference of phase. The column headed $\delta\,(=\theta_2-\theta_1)$ gives the *uncorrected* difference of phase in degrees, while that headed $\delta-\chi$ gives the *corrected* difference. The columns headed δ/π and $(\delta-\chi)/\pi$ give the corresponding phase differences as fractions of the half wave-length.

ϕ.	δ.	$\delta-\chi$.	δ/π.	$(\delta-\chi)/\pi$.
0°	0° 0′	0° 0′	0	0
30	7 24	7 14	0·0411	0·0402
40	13 54	13 34	0·0772	0·0754
50	23 42	23 6	0·1317	0·1283
60	39 11	38 6	0·2177	0·2116
70	66 54	64 51	0·3718	0·3604
75	89 16	86 37	0·4960	0·4814
80	118 6	115 24	0·6561	0·6410
90	180 0	180 0	1	1

Unfortunately, Conroy did not observe the differences of phase, so that we are unable to put these numbers to the test of agreement with experiment. From our formulæ we see that the phase differences will vary with M and α, and so will depend on the optical quality of the steel and on the nature of the light employed.* For want of other data we shall compare our formulæ with the experimental results obtained by M. de Senarmont, taking

* *Cf.* the results of a long series of experiments by M. Mouton, quoted in Mascart's 'Traité d'Optique,' t. 2, p. 542.

$M^2 = 15{\cdot}5$ and $\alpha = 54°$. With these constants we get the following values for δ:—

ϕ	30°.	40°.	50°.	60°.	70°.	75°.	80°.
δ	6° 48′	12° 29′	21° 44′	35° 55′	61° 38′	83° 10′	119°

The Principal Incidence was very close to that obtained by Conroy, and the values of M and α are near those that we have found to correspond closely with the results of Conroy's experiments. We might expect, then, that the corrections due to the layer of transition would not differ much from those found for Conroy's steel. Taking them to be the same, we get the following table, in which the theoretical results are compared with the experimental, and the differences noted.

ϕ.	$(\delta-\chi)$.		Difference.	$(\delta-\chi)/\pi$.		Difference.
	Theory.	Experiment.		Theory.	Experiment.	
30°	6° 38′	6° 37′	+0° 1′	0·0369	0·0368	+0·0001
40	12 9	12 0	+0 9	0·0675	0·0667	+0·0008
50	21 8	20 38	+0 30	0·1174	0·1147	+0·0027
60	34 50	32 6	+2 44	0·1936	0·1783	+0·0153
70	59 35	56 59	+2 36	0·3311	0·3165	+0·0146
75	80 31	80 46	−0 15	0·4472	0·4489	−0·0017
80	116 18	116 42	−0 24	0·6463	0·6485	−0·0022

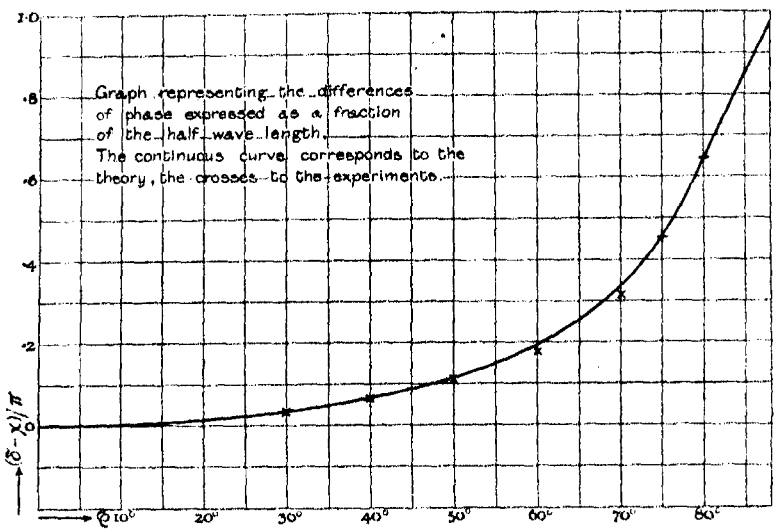
These results are exhibited graphically in fig. 6 (p. 230), and it will be seen that there is a satisfactory agreement between theory and experiment.

Thickness of the Transition Layer.

If d be the thickness of the layer, we have $d_1 = 2\pi d/\lambda_0\mu_0$, where μ_0 is the coefficient of refraction of the medium in contact with the metal, and λ_0 the wave-length in that medium.

Also we have $ae^{iw} = id_1\mathrm{E}\mu^2$, so that $a = d_1\mathrm{E}_1\mathrm{M}^2$, where E_1 is the modulus of E. From these relations we see that in order to determine the thickness of the layer we must know E_1 as well as a and M^2. As the value of E_1 depends on the law of variation of μ^2, we cannot determine it without a knowledge of the physical condition of the layer. We have seen, however (p. 218), that we should expect E_1 to lie between 1 and M^2. This would make d_1 lie between a/M^2 and a/M^4.

FIG. 6.



With the constants adopted above in the case of steel when discussing Conroy's experiments, this makes d_1 lie between 0·0132 and 0·001. The upper limit is near that derived from Kurz's experiments on reflection from glass into air.* It gives $d/\lambda = 0{\cdot}0021$.

If μ^2 in the layer obeyed any simple law there would be no difficulty in calculating E. Suppose, for example, that $\mu^2 = [\mu_0^2+(M^2-\mu_0^2)x]\,e^{-i2\alpha x}$ which gives the correct values of μ^2 at the faces of the layer. We should then have

$$E_1e^{i\omega} = E = \int_0^1 \mu^2 dx = \int_0^1 [\mu_0^2+(M^2-\mu_0^2)x]\,e^{-i2\alpha x}dx$$

$$= \left[\frac{M^2\sin 2\alpha}{2\alpha}-\frac{M^2-\mu_0^2}{2}\left(\frac{\sin\alpha}{\alpha}\right)^2\right]+i\left[\frac{M^2-\mu_0^2}{2}\left(\frac{1-\sin 2\alpha/2\alpha}{\alpha}\right)-M^2\frac{\sin^2\alpha}{\alpha}\right].$$

Whence

$$E_1^2 = \left[\frac{M^2\sin 2\alpha}{2\alpha}-\frac{M^2-\mu_0^2}{2}\left(\frac{\sin\alpha}{\alpha}\right)\right]^2+\left[\frac{M^2-\mu_0^2}{2}\left(\frac{1-\sin 2\alpha/2\alpha}{\alpha}\right)-\frac{M^2\sin^2\alpha}{\alpha}\right]^2,$$

and $$\tan\omega = \frac{(M^2-\mu_0^2)(1-\sin 2\alpha/2\alpha)-2M^2\sin^2\alpha}{M^2\sin 2\alpha-(M^2-\mu_0^2)\sin^2\alpha/\alpha}.$$

In Conroy's steel we have

$$M^2 = 13;\quad \alpha = 0{\cdot}9372 \text{ radian } (= 53^\circ\, 42').$$

This gives for air,

$$\mu_0 = 1,\qquad E_1 = 6{\cdot}261,\quad \omega = 110^\circ\, 24'$$

* See 'Roy. Soc. Proc.,' A, vol. 76, p. 58.

and for water,

$$\mu_0 = 1{\cdot}33, \quad E_1 = 6{\cdot}552, \quad \omega = 112^\circ \;\; 5'.$$

In the case of air we get $w = 90^\circ + \omega - 2\alpha = 93^\circ$, which is within half a degree of the value derived (on p. 224) from Conroy's experiments. The corresponding value of E_1 makes $d_1 = 0{\cdot}0021$ and $d/\lambda = 0{\cdot}0003$.

Effect of Changing the Medium in Contact with the Metal.

It has been found by experiment that the Principal Incidence and Principal Azimuth depend not only upon the nature of the reflecting metal, but also upon the medium in contact with it. Quincke* made some investigations into this matter, but the subject has been discussed much more completely in a long series of experiments on gold and silver by Sir John Conroy.† Conroy could find no simple relation between the changes in the values of the Principal Azimuth and Incidence and the indices of the media. His results, however, are in complete accordance with the trend of this paper, as is also the observation of Drude that surface impurities tend to reduce the value of the Principal Incidence.‡

If the medium in contact with the metal be of refractive index μ_0, we have to replace M in our earlier formulæ by M/μ_0, keeping α as before. We have seen that for an *abrupt* transition the Principal Incidence is given by the formula $\sin^4\phi = M^2c^2\cos^2\phi$, which is approximately equivalent§ to $\sec\phi = M + M^{-1}(1 - \frac{1}{2}\cos 2\alpha)$. If there be a layer of transition we have the equation

$$\sin^4\phi = M^2c^2\cos^2\phi + 2Mc\cos\phi\sin^2\phi\sin(\alpha+u)\tan\chi.$$

Now $\tan\chi$ is always small, so that in the last term we may put $\sin^2\phi = Mc\cos\phi$ and we then get

$$\sin^4\phi = M^2c^2\cos^2\phi\,(1+2\kappa),$$

where $\kappa = \sin(\alpha+u)\tan\chi$, and is small.

Thus the effect of the layer is to replace M^2 by $M^2(1+2\kappa)$ or M by $M(1+\kappa)$, while the effect of replacing air by a medium of refractive index μ_0 is to replace M by M/μ_0.

Hence, if there be a layer of transition between a medium μ_0 and the metal, the Principal Incidence will be determined by the equation

$$\sec\phi = M' + M'^{-1}(1 - \tfrac{1}{2}\cos 2\alpha), \quad \text{where} \quad M' = M(1+\kappa)\mu_0^{-1}$$

* 'Pogg. Ann.,' vol. 188, p. 541.

† See 'Roy. Soc. Proc.,' vol. 31, p. 486.

‡ See Drude, 'Wied. Ann.,' vol. 36, 1889, and vol. 39, 1890.

§ See p. 218.

An increase of μ_0 will diminish M′ and so diminish ϕ, the Principal Incidence.

The following are the means of Conroy's experimental determinations of the Principal Incidences, the incident light being yellow:—

	°	′		°	′
Gold in air	71	43	Silver in air	74	37
„ water	67	39	„ water	72	15
„ carbon bisulphide	66	36	„ carbon tetrachloride	71	39

A change in μ_0 will also affect the Principal Azimuth (β). We have

$$\tan\beta = (1-2q\cos\theta+q^2)^{\frac{1}{2}}\tan\tfrac{1}{2}(\alpha+u)$$

$$= (1-q\cos\theta)\tan\tfrac{1}{2}(\alpha+u), \text{ very nearly.}$$

The angle u is determined by the equation

$$\cot 2u = \frac{M^2/\mu_0^2}{\sin^2\phi\sin 2\alpha} - \cot 2\alpha.$$

An increase of μ_0 will diminish $\cot 2u$ and therefore increase u, so that as a rule β will be increased slightly, although in some cases the increase of $\tan\frac{1}{2}(\alpha+u)$ may be counterbalanced by the diminution of the factor $1-q\cos\theta$.

These are the means of Conroy's experimental determinations of the Principal Azimuth, corresponding to the Principal Incidences above:—

	°	′		°	′
Gold in air	41	14	Silver in air	43	22
„ water	41	15	„ water	44	9
„ carbon bisulphide	41	41	„ carbon tetrachloride	43	40

It will be observed that these results are in general accord with the argument above. Unfortunately, however, we have not sufficient data to put the theory of a layer of transition to the exact test of numerical verification or otherwise. We have seen that a knowledge of the Principal Incidence and the Principal Azimuth is not enough to determine the optical properties of a metal, and Conroy's results do not enable us to supply the deficiency. Even if we had sufficient data to determine the constants a and w for air, we could still do little better than guess what they would become when some other medium was in contact with the metal. We have $E = \int_0^1 \mu^2 dx$, so that the modulus of E should be increased by an increase of μ_0.*

* *Cf.* p. 230.

Hence an increase of μ_0 might be expected to raise a and therefore also χ and κ. These quantities χ and κ will be further raised when M is replaced by M/μ_0, for this will increase θ', and so increase $\sin\theta$ and diminish $-\cos\theta$. We should expect, then, that the diminution of ϕ, due to the increase of μ_0, would be greater if there were a layer of transition than if there were none. The effect on the Principal Azimuth (β) is not so easily described. An increase of μ_0 will raise q, but it will diminish $\cos\theta$, so that we cannot say in general whether $1-q\cos\theta$ will be increased or diminished.

A little investigation will show that Conroy's experimental results are not consistent with the theory of an abrupt transition from one medium to the other. Thus for gold in air with yellow light his values of the Principal Azimuth and Incidence would give $M = 2{\cdot}719$ and $\alpha = 81^\circ\ 33'$ on the theory of an abrupt transition. For gold in water ($\mu_0 = 1{\cdot}33$) these constants would lead to $\phi = 68^\circ\ 49'$ and $\beta = 41^\circ\ 30'$, whereas Conroy found $\phi = 67^\circ\ 39'$ and $\beta = 41^\circ\ 15'$. For gold in carbon bisulphide ($\mu_0 = 1{\cdot}63$) theory would give $\phi = 66^\circ\ 58'$ and $\beta = 41^\circ\ 46'$ instead of $\phi = 66^\circ\ 36'$ and $\beta = 41^\circ\ 41'$ as obtained by Conroy. With different colours for gold and also for silver the same discrepancy between theory and experiment would also be apparent, the differences being in nearly every case in the same direction, the theoretical results being too large. This discrepancy is just what the above discussion would lead us to expect, if there is a layer of transition between the two media.

Summary.

The chief results of the present investigation are the following:—

1. That in metallic reflection, if the transition from one medium to the other be abrupt, the Principal Incidence is always near the quasi-polarising angle, and is given very approximately by the formula

$$\sec\phi = M + M^{-1}(1-\tfrac{1}{2}\cos 2\alpha).$$

2. That even when great care is taken to clean the surface of a metal the transition from it to the neighbouring medium is often gradual and not abrupt. This is in accordance with experimental and theoretical investigations on reflection from *transparent* substances such as glass and diamond.

3. That the influence of this layer on the ellipticity of the reflected light and on the difference of phase between light polarised perpendicularly and parallel to the plane of incidence extends over a wider range than in the case of transparent substances.

4. That the thickness of the layer is of about the same order of magnitude as with transparent media.

5. That the layer affects the position of the Principal Incidence considerably, and also influences the Principal Azimuth.

6. That, consequently, the deduction of the optical constants of a metal from observation of the Principal Incidence and Azimuth alone is liable to considerable error. [In the case of steel this method leads to $\mu = 2{\cdot}249$ and $\alpha = 3{\cdot}257$ (see p. 216), while the wider theory yields $\mu = 2{\cdot}134$ and $\alpha = 2{\cdot}906$.]

7. That four constants are required to describe the optical properties of a metallic reflector, two of them depending on the nature of the layer of transition.

8. That with these four constants a very satisfactory agreement exists between theory and experiment, as regards both the intensity of the reflected light and the difference of phase between the lights polarised perpendicularly and parallel to the plane of incidence.

The Relation Between the Osmotic Pressure and the Vapour Pressure in a Concentrated Solution.

By Will Spens, B.A., King's College, Cambridge.

(Communicated by W. C. D. Whetham, F.R.S. Received November 11, 1905; received in Revised form January 3,—Read January 25, 1906.)

1. The relation between the vapour pressure and the osmotic pressure of a solution is often investigated by considering the equilibrium of a column of solution separated at the bottom from the pure solvent by a semi-permeable membrane, and placed in an atmosphere of vapour from the solvent. Now the hydrostatic equilibrium of the vapour column gives

$$\delta p = gs^{-1}\delta h,$$

where p is the vapour pressure of the pure solvent, g the acceleration due to gravity, h the height above the surface of the pure solvent, and s the specific volume of the vapour. Hence considering the equilibrium of the liquid column we get

$$\mathrm{P}+p-p' = \rho hg = \rho\int_{p'}^{p} s\,dp,$$

when P is the osmotic pressure, p' the vapour pressure of the solution, p that of the pure solvent, and where ρ is the effective mean density of the column of liquid.

The writer was led to doubt whether this method could be applied to concentrated solutions, by the fact that a different result is obtained when the same problem is attacked by the consideration of an isothermal cycle in accordance with Van't Hoff's method. This method, when fully worked out, seems to lead to the equation :—

$$\mathrm{P}v_s + pv_s - p'v_s' + \mathrm{W} - c\frac{d\mathrm{W}}{dc} = \int_{p'}^{p} s\,dp,$$

where P, p, p' and s have their previous meanings, v_s is the increment in volume of a large mass of solution at a hydrostatic pressure $\mathrm{P}+p$ when unit mass of solvent is added, v_s' the increment in volume of a large mass of solution at a hydrostatic pressure p' when unit mass of solvent is added, W is the work done in compressing unit mass of solution from a hydrostatic pressure p' to a hydrostatic pressure $\mathrm{P}+p$, and c is the concentration measured by the mass of solute in unit mass of solution.

In considering the discrepancy between these two results the writer had come independently to the same conclusion as the Earl of Berkeley and Mr. Hartley, namely, that in the column method error is introduced through neglect of variation, owing to the action of gravity, of the concentration at different levels. As however the writer's conclusions differ, in some possible cases notably, from theirs, the following investigation is submitted.

2. In the column method what is undoubtedly found is a perfectly rigid result connecting the osmotic pressure corresponding to the concentration at the bottom of the column and the lowering of vapour pressure corresponding to the concentration at the top. In order therefore to apply the result to a comparison of the osmotic pressure and vapour pressure of solutions at the same concentrations, it becomes necessary to obtain and employ a correction that will give the osmotic pressure corresponding to the concentration at the top in terms of the known osmotic pressure corresponding to the concentration at the bottom. It is to be noted that in all cases osmotic pressures must be computed for equilibrium between solution and pure solvent under one definite pressure, say that of the vapour, for, as Duhem has pointed out,* the osmotic pressure of a solution, defined as the difference in the hydrostatic pressures of solution and solvent when in equilibrium through a semi-permeable membrane, is a function not only of the concentration of the solution but also of the hydrostatic pressure of the solvent. Occasion will arise to return to this point, since, as will be shown, on it depends the discrepancy between the results of the Earl of Berkeley and Mr. Hartley and of the writer.

* 'Mécanique Chimique,' vol. 3, p. 64.

A thermodynamic argument which seems to satisfy these considerations is as follows: Consider a column of solution of such height as to be in equilibrium with pure solvent through a semi-permeable membrane at its base, and further suppose a large flat reservoir at the level of the summit and filled with solution at the concentration of the solution there. Let the following isothermal reversible cycle be performed:—

Let the top of the column of solution be covered with a piston which exerts on the solution a hydrostatic pressure equal to the vapour pressure. Let the semi-permeable membrane also be part of a piston, and the pure solvent be covered by a piston exerting on it a pressure equal to its vapour pressure. Suppose all the pistons to be weightless.

Then (1) keeping the piston covering the solution fixed, move the piston separating solution and solvent so as to remove δm of solvent from the solution, the piston covering solvent being free to move. Then work done by the system is $-[P_1 D - p\ (u-D)]\ \delta m$, where P_1 is the osmotic pressure corresponding to the concentration at the bottom of the column, $D\delta m$ is the loss in volume of the column, p is the vapour pressure, and u the specific volume of the solvent. The operation being infinitely slow, the solvent will be drawn from throughout the solution and the process will be reversible.

(2) Cover the semi-permeable membrane between solution and solvent by a shutter; then connect the reservoir at the top with the column through a semi-permeable membrane. Further, let the reservoir be closed by a piston exerting on the solution in it a pressure equal to its vapour pressure. Now, since the volume of the columns may be as large as is desired, we may neglect variation of the concentrations due to the above-described loss of δm of solvent, and therefore the solution at the top of the column and in the reservoir may be taken as still at the same concentration. Hence, if the piston at base of column be fixed, that at top be free to move and that between top and reservoir be moved so as to transfer δm of solvent from reservoir to column, there is no osmotic work, and work done by the system $= \delta m\ p'\ (D - v_s')$, where p' is the vapour pressure of solution at top of column or in reservoir, and v_s' is the increment in volume of solution at that concentration, and at a pressure p' on adding unit mass of solvent to a large amount of solution.

(3) Disconnect the reservoir from the column and compress the solution in the reservoir to a pressure $P_2 + p$, where P_2 is the osmotic pressure for the solution in the reservoir (since this may be of any size, P_2 is also the osmotic pressure corresponding to the concentration at the top of the column). The work done by the system is $-$ MW, where M is mass of solution in the

reservoir, and W the work done in compressing unit mass from a pressure p' to a pressure $P_2 + p$.

(4) Connect the reservoir through a semi-permeable piston with pure solvent, and by moving piston introduce δm of solvent into reservoir. The pure solvent is supposed to be covered by a piston exerting on it a hydrostatic pressure equal to its vapour pressure, and the work done by the system will thus be $[P_2 v_s - p(u-v_s)]\,\delta m$, where v_s differs from v_s' in being for solution at a pressure $P_2 + p$ and not p'.

(5) Lower the column of solution through a height $D\delta m/a$, where a is the sectional area of the column, and raise a mass δm of solvent through the height h where h is the height of the column. The work done by the system is equal to the loss in energy of position, that is $hg\rho a\,D\delta m/a - hg\delta m$ or $-hg(1-\rho D)\,\delta m$ where ρ is the effective mean density of the column. But $hg\rho = P_1 + p - p'$, since that is the condition for equilibrium across a semi-permeable membrane at the foot of the column. Hence the work is $-(1-\rho D)(P_1+p-p')\,\rho^{-1}\delta m$.

(6) Release the pressure on the solution in the reservoir to p', then work done by the system is $+\{MW + \delta m.d/dm\,(MW)\}$ where m is mass of solvent, that is $+\{MW + W\delta m - \delta m c dW/dc\}$, c being as before mass of solute in unit mass of solution.

The above is a complete cycle in all essentials, and being isothermal and reversible we may collect the work and equate to zero. Hence

$$\begin{aligned} 0 = &-[P_1 D - p(u-D)]\delta m + \delta m p'(D - v'_s) - MW + [P_2 v_s - p(u-v_s)]\,\delta m \\ &-(1-\rho D)(P_1+p-p')\,\{\rho^{-1}\delta m + MW + W\delta m - \delta m c dW/dc\}, \end{aligned}$$

whence

$$(P_1+p-p')\rho^{-1} = (P_2+p)v_s - p'v_s' + W - c\,dw/dc.$$

Hence, if there be a column of solution whose top surface is under a pressure equal to the vapour pressure, and which is in equilibrium at its base, through a semi-permeable membrane, with pure solvent (at a pressure equal to its vapour pressure), this equation gives the connection between the osmotic pressures, corresponding to solution at the top and bottom of the column, computed in each case with reference to solvent under the pressure of its vapour.

Such a column is identical with that used in deducing the relation between osmotic pressure and vapour pressure, and we have

$$P_1 + p - p' = hg\rho = \rho\int_{p}^{P} s\,dp,$$

on substituting from the above,

$$(P_2+p)v_s - p'v_s' + W - c\frac{dW}{dc} = \int_{p}^{P} s\,dp,$$

which is identical with the result previously quoted as obtainable by Van't Hoff's method.

Since $v_s - v_s'$ is in general very small as also W *and* $c d\mathrm{W}/dc$, *and since $p - p'$ is negligible compared with* P, *the result reduces to the approximate equation*

$$\mathrm{P}v_s = \int_{p'}^{p} s\,dp,$$

or assuming Boyle's law to hold for the vapour,

$$\mathrm{P}v_s = sp \log p/p'.$$

3. The result deduced by Lord Berkeley and Mr. Hartley is

$$\mathrm{P}u = sp \log p/p',$$

where u is the specific volume of the solvent.

Since v_s is the increment in volume of a large mass of solution when unit mass of the solvent is added, these equations will only be identical when this increment is equal to the volume of solvent added, that is when the contraction on dilution is negligible.

In many cases, *e.g.*, cane sugar dissolved in water, this will be the case up to considerable concentrations, but in other cases it will not be so; thus in solutions of caustic soda in water v_s and u differ appreciably at comparatively moderate concentrations, and the difference rises to 14 per cent. in a 50-per-cent. solution.* *If, therefore, the relation between osmotic pressure and vapour pressure is to be applied in the case of concentrated solutions, it is necessary to determine v_s, and if v_s differs appreciably from u, to employ v_s and not u.*

4. As has already been stated, the presence of u for v_s in the equation due to Lord Berkeley and Mr. Hartley depends on the assumption that the osmotic pressure (*i.e.*, the difference in hydrostatic pressure between solution and solvent when in osmotic equilibrium) is independent of the hydrostatic pressure of the solvent and, for example, would not change if the whole system was compressed in a hydrostatic press.

In order to show that the removal of this assumption leads to the writer's

* The following figures are based on a table published ('Phil. Trans.,' Series A, vol. 204, p. 273) by Mr. W. R. Bousfield and Dr. Lowry for a solution at 18° C. :—

Percentage NaOH.	v_s.	Percentage NaOH.	v_s.
5	0·9979	30	0·9333
10	0·9908	35	0·9120
15	0·9804	40	0·8998
20	0·9671	45	0·8724
25	0·9524	50	0·8585

The specific volume of water at 18° was 1·00134.

result it is necessary to obtain an equation for the variation in osmotic pressure when solution of constant concentration is successively in equilibrium with solvent at two different hydrostatic pressures. This relation can be obtained as follows by considering an isothermal reversible cycle performed on a system, consisting of a large mass of solution, connected through a semi-permeable piston with a large mass of solvent. Suppose solution and solvent to be confined by pistons through which pressures can be applied.

(1) Apply to the solvent a pressure π_1 and such a pressure to the solution as will give osmotic equilibrium. Let this pressure be $P_1+\pi_1$, so that P_1 is the osmotic pressure. Then the work done by the system, if the piston confining the solution be kept stationary and that between solution and solvent moved so as to introduce a mass δm of solvent into the solution, is $[P_1 v_{s1}-\pi_1(u_1-v_{s1})]\,\delta m$, when u_1 is the specific volume of the solvent at a pressure π_1 and v_{s1} is the increment in volume of a large mass of solution at a pressure $P_1+\pi_1$ on the addition of unit mass of solvent.

(2) Compress the solvent to a pressure π_2, simultaneously compressing the solution in such a way as to maintain osmotic equilibrium. Let $P_2+\pi_2$ be the final hydrostatic pressure of the solution, so that P_2 is then the osmotic pressure when the solvent pressure is π_2. If W be the work done in compressing unit mass of solution from $P_1+\pi_1$ to $P_2+\pi_2$, M being the mass of solution, W′ the work done in compressing unit mass of solvent from π_1 to π_2, and M′ the mass of the solvent. Work done by the system $-(MW+M'W')$.

(3) Move the semi-permeable piston so as to remove a mass δm of solvent from the solution, keeping stationary the piston confining the solution. The work done by the system is $[-P_2 v_{s2}+\pi_2(u_2-v_{s2})]\,\delta m$, where u_2 is the specific volume of the solvent at a pressure π_2, v_{s2} is the increment in volume of a large mass of solution at a pressure π_2+P_2 when unit mass of solvent is added.

(4) Release pressure on solvent to π, releasing that on solution so as to maintain equilibrium. The work done by the system is equal to

$$\left(MW-\delta m\frac{d(MW)}{dm}+M'W'+\delta m W'\right), \text{ or } MW+M'W'+\delta m\left(W'-W+c\frac{dW}{dc}\right).$$

The above is a complete isothermal reversible cycle.

Collecting the terms for the work and equating to zero we have

$$\begin{aligned}0 = [P_1 v_{s1}-\pi_1(u_1-v_{s1})]\,\delta m - MW - M'W' \\ -[P_2 v_{s2}-\pi_2(u_2-v_{s2})]\,\delta m + MW + M'W' + \delta m\,(W'-W+c\,dW/dc),\end{aligned}$$

whence

$$P_2 v_{s2}-\pi_2(u_2-v_{s2}) = P_1 v_{s1}-\pi_1(u_1-v_{s1})+W'-W+c\,dW/dc.$$

Since in general, $W' - W + c\,dW/dc$, $v_{s2} - v_{s1}$, $u_2 - u_1$, will be quite negligible, we may neglect change of volume due to compression and write

$$(P_2 - P_1)\,v_s = (\pi_2 - \pi_1)(u - v_s) \quad \text{or} \quad \frac{dP}{d\pi} = \frac{u}{v_s} - 1\,.$$

Now in the deduction, due to Lord Berkeley and Mr. Hartley, of the variation of the osmotic pressures corresponding to the concentrations at different levels in a column of solution, it is shown that if P' be the osmotic pressure when solution at the top of a column is connected through a semi-permeable membrane with solvent at a pressure p (that of its vapour), and if P be the osmotic pressure when solution at the bottom of the column (height h) is in equilibrium with solvent at a pressure $p + \rho_0 gh$, where ρ_0 is density of the solvent,

$$P - P' = (\rho - \rho_0)\,gh,$$

where ρ is the mean density of the column of solution. Let P_0 be the value to which P would change if the solvent pressure changed from $p + \rho_0 gh$ to p, then $P - P_0 = \rho_0 gh\,(u/v_s - 1)$, or subtracting from the last equation and remembering that ρ_0 is equal to u^{-1}, we get

$$P_0 - P' = (\rho - v_s^{-1})\,gh.$$

But to the required degree of accuracy we have from the column method discussed by Lord Berkeley and Mr. Hartley,

$$P_0 = \rho gh = \rho sp \log p/p',$$

where P_0 is the osmotic pressure corresponding to a solution at the bottom of the column and a solvent pressure p, ρ is the mean density of the column, h its height, $p - p'$ the lowering of vapour pressure for solution at the top.

Hence if P be the osmotic pressure corresponding to this solution at the top (and a solvent pressure p),

$$P_0 - P = \rho gh - v_s^{-1} gh\,;$$

or, subtracting from the last equation,

$$P = v_s^{-1} gh \quad \text{or} \quad P v_s = sp \log p/p',$$

which is the writer's result.

In conclusion, I have to thank Mr. Whetham for very valuable criticism.

On the Effect of High Temperatures on Radium Emanation.

By WALTER MAKOWER, Harling Fellow of the University of Manchester.

(Communicated by Arthur Schuster, F.R.S. Received December 22, 1905,—Read January 25, 1906.)

1. *Introduction.*

It has been shown by Curie and Danne* that the rate of decay of radium C can be altered by subjecting it to temperatures above 630° C. The rate of decay increases with rise of temperature, reaching a maximum at about 1100° C., after which it again decreases up to 1300° C. The following are some of the values taken from their paper :—

Temperature centigrade.	c'.	θ.
630°	$3 \cdot 94 \times 10^{-4}$	29·3
830	$4 \cdot 70 \times 10^{-4}$	24·6
1000	$5 \cdot 50 \times 10^{-4}$	21·0
1100	$5 \cdot 70 \times 10^{-4}$	20·3
1250	$4 \cdot 80 \times 10^{-4}$	24·1
1300	$4 \cdot 50 \times 10^{-4}$	25·4

Here c' is the radio-active constant, and θ is the time in minutes taken by the activity of radium C to fall to half its value.†

H. L. Bronson‡ has more recently made some experiments on the same subject, and is led to the opposite conclusion. He states that the heating of the active deposit from radium emanation to temperatures between 700° and 1100° C. is without effect, and to explain both his results and those of Curie and Danne, he suggests that radium C has a shorter instead of a longer period than radium B, and that the latter is the more volatile of the two; part of the radium B is, however, in general supposed to have remained on his wires after heating.

In view of the conflicting evidence on the subject, it seemed desirable that further experiments on this important question should be undertaken. The following is an account of an investigation on the influence of temperature on the activity of radium emanation when in radio-active equilibrium with

* 'Comptes Rendus,' vol. 138, pp. 748–751, March, 1904.

† The nomenclature suggested by Rutherford is here adopted, in which radium C is the third product after the emanation.

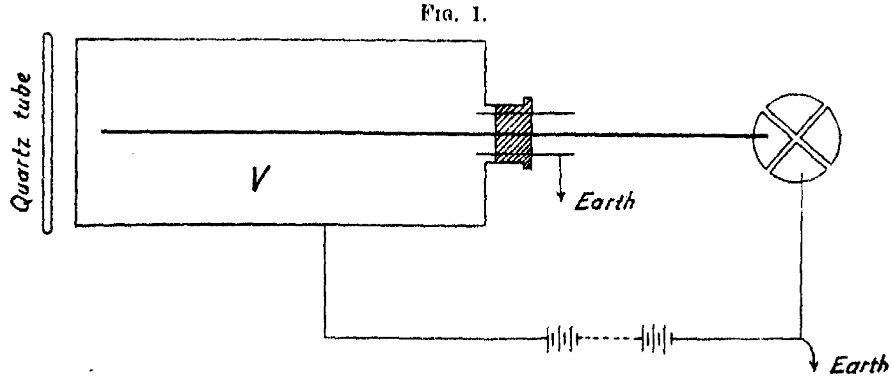
‡ 'Amer. Jour. Sci.,' pp. 60–64, July, 1905, and 'Phil. Mag.,' s. 6, vol. 11, No. 61, January, 1906, p. 143.

radium A B, and C, and when sealed up in a quartz tube so that there can have been no possibility of the escape of any volatile product. The results show clearly that the activity as measured by the β and γ rays can be changed by high temperatures, the observed effects being *consistent* with the explanation offered by Curie and Danne, that the rate of decay of radium C is increased by high temperatures. The experiments do not, however, *prove* that it is this particular product which is affected, as the results could be equally well explained if one or more of the other radio-active bodies present underwent some alteration. Further experiments will be necessary before this question can be definitely settled, but there can be no doubt that the rate of decay of one of the products is affected by temperature.

2. *Experimental Method.*

The emanation from about 5 milligrammes of radium bromide was collected in a small quartz tube 12 cm. long and 0·5 cm. diameter, closed at one end. The open end was connected to the bulb containing the radium, and the whole apparatus was evacuated, to allow the emanation to diffuse rapidly. The emanation was condensed in the tube by immersing it in liquid air, and sufficient air was admitted to cause the pressure inside the quartz-tube to be about atmospheric when raised to the highest temperatures subsequently to be used. The quartz-tube was then sealed off in an oxyhydrogen blow-pipe at a constriction near its upper end, and removed from the liquid air bath. Its radio-activity was tested by measuring the ionisation produced in a cylindrical metal vessel V (fig. 1) when the quartz-tube was placed on a

Fig. 1.



small wooden stand in the position shown in the figure. The sides of the quartz-tube and the bottom of the vessel V were together thick enough to absorb all the α rays coming from the emanation, but thin enough to let through a considerable portion of the β-rays and most of the γ-rays.

Observations of the ionisation in V therefore afforded a measure of the quantity of radium C present in the quartz-tube, since this is the only radio-active product which emits these rays in sufficient quantity to be of consequence. A saturation current was obtained from 200 small storage cells, and the measurements were made by means of a Dolezalek electrometer.

It was found that the quartz-tube could be removed from its stand and replaced so nearly in the same position that the ionisation in V did not change by an appreciable amount.

After leaving sufficient time for radio-active equilibrium to be established, the activity was measured. The quartz-tube was then placed in an electric furnace and heated to a high temperature for a definite time, the activity being again tested as soon as possible after the removal of the quartz-tube from the furnace.

3. *First Series of Experiments.*

In these experiments a carbon-tube furnace of the kind described by Hutton and Patterson* was used. No accurate measurements of temperature were made in this series of experiments, but the maximum temperature attained was estimated by placing wires of different metals in the furnace on either side of the quartz-tube, and noting which were fused.

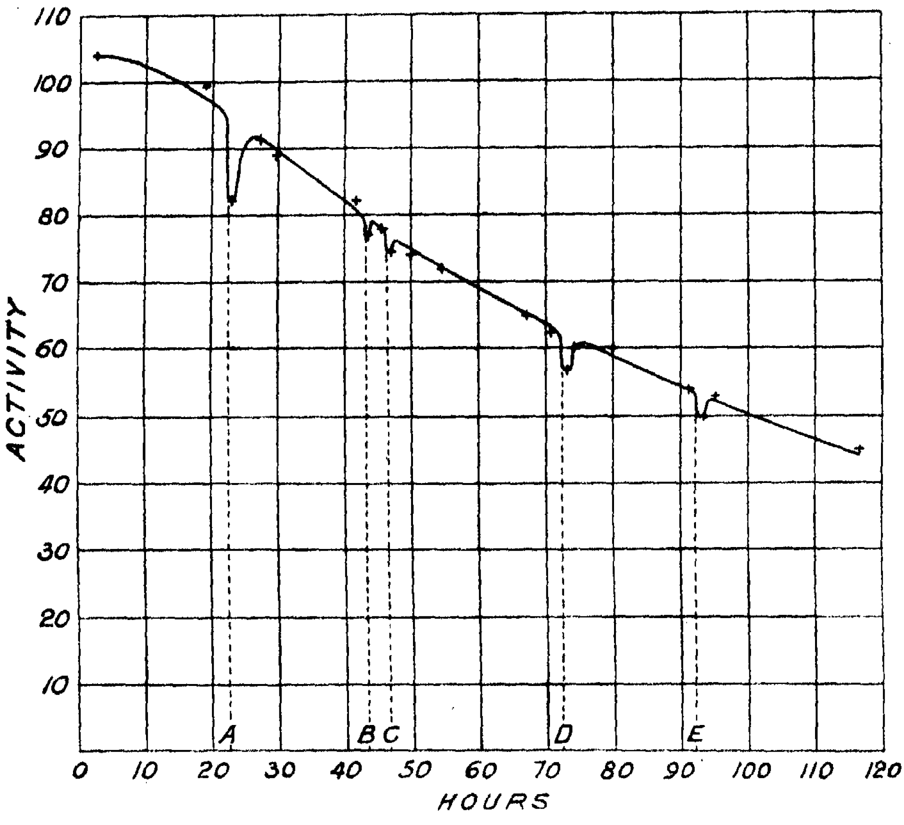
Details of the experiments are given in Table I. The results are also shown graphically in fig. 2, in which the ordinates represent, in arbitrary units, the radio-activity of the emanation, and the abscissæ give time in hours. The zero of time is taken at the moment of sealing of the quartz-tube. The points on the curve represent the means of several observations taken within a few minutes of each other, and the points A, B, C, D, E the times at which the quartz-tube was heated.

Table I.

Experiment.	Time of heating, in minutes.	Temperature between melting points of	Activity.		
			Before heating.	After heating.	Percentage fall.
A	15	Nickel and platinum ...	97	82	15·5
B	15	" " ...	82	77	6·1
C	15	Copper and nickel	78	75	3·85
D	31	—	62·5	57	8·8
E	24	Slightly above that of nickel	54	50	7·4

* 'Trans. Faraday Soc.,' 1905, vol. 1, pp. 187–196.

FIG. 2.



Leaving out of account the initial stage immediately after filling the tube, the activity of the emanation decreased according to an exponential law, falling to half in 3·7 days. It will, however, be noticed that the activity always fell temporarily after heating the emanation, the activity *recovering to its normal value* in about one hour. The latter point is of importance, as the observed decrease of activity might otherwise be supposed due to the porosity of the quartz when hot to the emanation; but this would leave unexplained the subsequent recovery of the activity to its normal value.

4. *Second Series of Experiments.*

As a result of the frequent heating in a carbon furnace, the quartz-tube used in the previous experiments crystallised and fell to pieces. A new and similar tube was therefore made, with which the following series of experiments was performed. The method was the same as in the first series, except that a small porcelain-tube furnace, heated by the passage of an

electric current through a nickel wire wound on it, was substituted for the carbon-tube furnace. The furnace was similar to those used by Dr. Harker at the National Physical Laboratory. Besides the fact that under these conditions the quartz-tube showed less tendency to crystallise, the advantage of the change consisted in the fact that the quartz-tube could be introduced into the furnace when a steady temperature had been reached, and could be quickly withdrawn while hot, and its activity measured within a few minutes after removal. Furthermore, the furnace temperatures were measured by a platinum platinum-rhodium thermo-couple standardised at the Reichsanstalt. The temperature of the furnace was very uniform, and remained constant to about 20° C. during an experiment.

The observations are given in Table II., and shown graphically in fig. 3. The general character of the results is similar to that obtained in the first series.

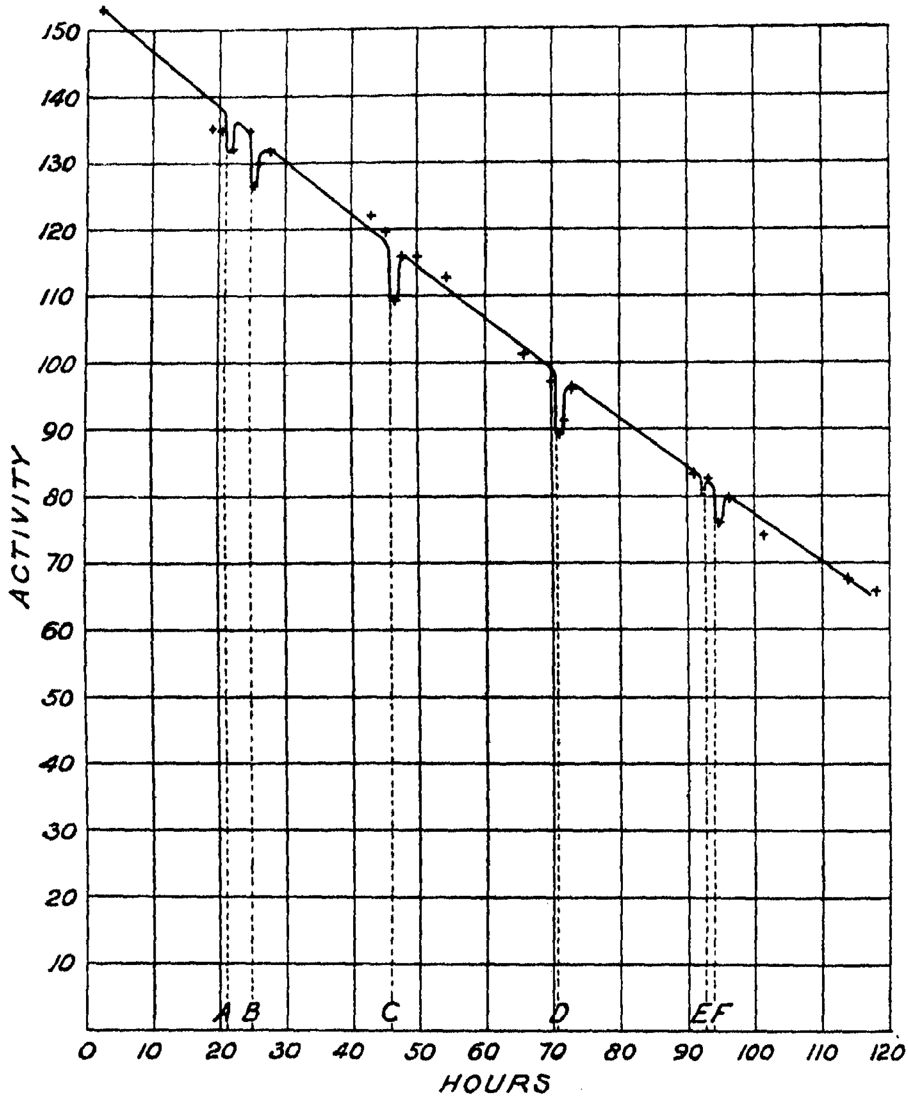
The change in activity increases with rise of temperature, being small at 1000° C. and increasing up to 1200° C. If the results are to be explained on the view expressed by Curie and Danne, the maximum change of activity should occur at 1100° C., and heating to a higher temperature should have less effect. This does not appear to be the case if the estimates of the temperatures in the first series of experiments can be trusted. Accurate measurements above 1200° C. will shortly be undertaken with a view to settling the question.

The observations D, E, F were made after heating the quartz-tube to nearly the same temperature. Heating for 10 minutes produced little, if any, effect, which certainly did not persist for more than a few minutes. Heating for two hours was, however, no more effective than heating for one hour. This is just what would be expected if it is the radium C which is affected.

Table II.

Experiment.	Time of heating, in minutes.	Temperature centigrade.	Activity.		
			Before heating.	After heating.	Percentage fall.
A	60	1000°	138	132	4·35
B	60	1100	134	126·5	5·6
C	60	1220	118	109	7·6
D	60	1185	98	89·5	8·7
E	10	1190	83	80	3·6
F	114	1200	81	76	6·2

Fig. 3.



5. *Summary of Results.*

(1) The activity of radium emanation in radio-active equilibrium with its products A, B, and C, is changed by heating above 1000° C.

(2) The effect increases with the temperature up to 1200° C., and possibly beyond this temperature.

(3) The effect increases with the time of heating for about the first hour, but subsequent heating is without effect.

In conclusion, I have to thank Professor Schuster for lending me the radium used in these experiments. To Dr. Hutton I am indebted for the kind way in which he placed his electric furnaces at my disposal, and for his advice as to the best methods of obtaining the temperatures required.

Polarisation in Secondary Röntgen Radiation.

By CHARLES G. BARKLA, D.Sc. (Liverpool), M.Sc. (Victoria), B.A. (King's College, Cambridge), Demonstrator and Assistant Lecturer, University of Liverpool.

(Communicated by Professor J. J. Thomson, F.R.S. Received January 17,—Read February 8, 1906.)

In a paper on "Polarised Röntgen Radiation,"* the writer gave an account of experiments which demonstrated the partial polarisation of a beam of X-rays proceeding from the antikathode of an X-ray focus-tube, and verified the theory previously given† of the production of secondary X-rays in light substances.‡

In that paper it was shown that the secondary radiation proceeding in a direction perpendicular to that of propagation of the primary radiation from certain substances placed in that primary beam should, according to the theory put forward, be plane polarised. From gases, however, the secondary radiation was not sufficiently intense to produce a tertiary of measurable intensity, and thus the polarisation of the secondary from them was not verifiable. On the other hand, though heavy metals were found to emit secondary radiation of sufficient intensity and ionising power to produce appreciable tertiary effects, in these metals the production of secondary radiation is a more complex phenomenon, and evidence of polarisation of the secondary beam is not to be expected from experiments upon them.

For the secondary radiator a substance had to be chosen which emitted a radiation of considerable intensity, yet differing very little in character from the primary. It had been shown that from such substances the intensity of radiation is proportional merely to the quantity of matter passed through by the primary of given intensity. A substance permitting the passage of the

* 'Phil. Trans.,' A, vol. 204, 1905, pp. 467—479.

† J. J. Thomson, 'Conduction of Electricity through Gases,' p. 268; C. G. Barkla, 'Phil. Mag.,' June, 1903, and May, 1904.

‡ More precisely, substances of low atomic weight.

primary beam through the greatest mass was therefore the most suitable for the experiment, that is a substance absorbing the radiation as little as possible. As the absorption per unit mass diminishes with the atomic weight,* the less the atomic weight of the substance the greater is the energy of the primary beam transformed into energy of secondary radiation. Preliminary experiments showed that it was possible, by using carbon as the radiator, to produce a secondary beam of X-rays of great intensity and capable of setting up a tertiary giving quite an appreciable ionisation in air.

The following experiments were then undertaken in order if possible to produce and give proof of an almost complete polarisation in a beam of Röntgen rays, and thus to further verify the theory of the production of secondary X-rays in substance of low atomic weight :—

A mass of carbon was placed near an excited X-ray tube so as to be subject to a primary beam of considerable intensity. It was then the source of a secondary radiation, the total energy of which was quite a large fraction of the energy incident upon it. A beam of this secondary radiation proceeding in a direction perpendicular to that of propagation of the primary falling on the carbon was studied.

In this secondary beam was placed a second mass of carbon, and the intensities of tertiary radiation proceeding in two directions at right angles and perpendicular to the direction of propagation of the secondary beam were observed by means of electroscopes placed in its path. The X-ray tube was turned round the axis of the secondary beam, while everything else was fixed, and the relative intensities of the tertiary radiations observed for different positions of the tube.

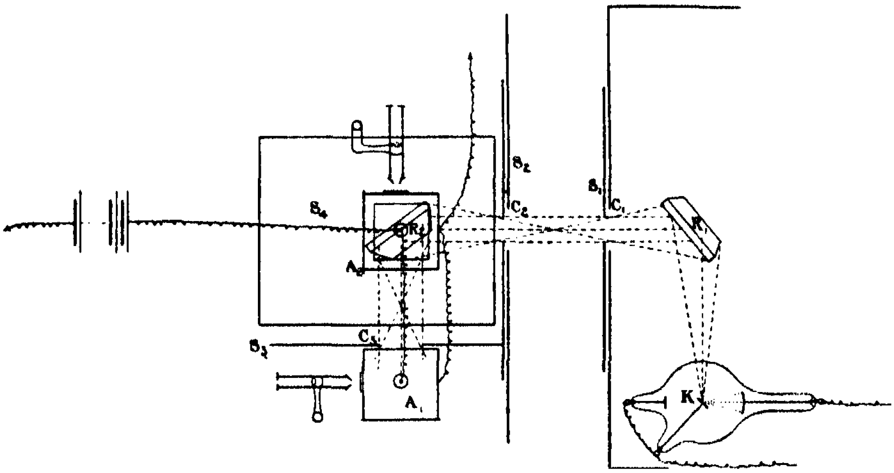
It was found that the intensity of tertiary radiation reached a maximum when the directions of propagation of the primary and tertiary were parallel, and a minimum when they were at right angles, showing the secondary radiation proceeding from carbon in a direction perpendicular to that of propagation of the incident primary to be polarised. As shown below, the amount of polarisation was enormous in comparison with what had been found in the primary beam proceeding direct from an X-ray tube, and indicated almost complete polarisation of the secondary beam.

The details of the experiments are given below.

A thick square plate of carbon ($8 \times 8 \times 1{\cdot}2$ cm.) and an X-ray tube were place inside a large lead-covered box in positions shown in the figure. The faces of the plate (which was near a rectangular aperture C_1 in the side of the box) were equally inclined to vertical and horizontal lines parallel to the sides of the box, and the face near the aperture was exposed to radiation

* Benoist, 'Journal de Physique' [3], vol. 10, p. 653, 1901.

from the tube. This was so placed that the line (about 16 cm. long) joining the centres of the antikathode and the carbon plate was parallel to the side of the box. The size of the aperture C_1 was adjustable by lead shutters S_1 placed just outside. Large screens S_2 of thick sheet lead were placed at a distance of 10 cm. from these shutters and parallel to the side of the box, so that the width of the aperture between them was also adjustable. The secondary beam passing through the aperture C_2 was then the beam whose polarisation was to be tested. Beyond S_2, and in a vertical plane perpendicular to the screen S_2, was another screen S_3, containing a square aperture C_3 (5 × 5 cm.), distant about 12 cm. from the centre of the radiator R_2, situated in the secondary beam about 10 cm. beyond the screens S_2. No secondary radiation from R_1 was incident upon the aperture C_3, for it was screened from



Plan of apparatus, showing position of bulb giving maximum deflection of electroscope A_1 and minimum of electroscope A_2.

R_1 by lead plates as shown in the figure. But tertiary radiation proceeding from the radiator R_2 passed through the aperture C_3. The beam which entered the electroscope A_1,* through a thin paper and aluminium face placed immediately behind this aperture, consisted then of radiation whose direction of propagation was approximately horizontal and perpendicular to the direction of propagation of the secondary beam. A similar lead screen S_4, and a brass plate which supported another electroscope A_2, were placed in horizontal planes above the secondary beam in such positions that the centres of the apertures were vertically above the centre of the radiator R_2, and distant about 12 cm. from it. Tertiary radiation proceeding in a vertical

* For description, see paper on "Polarised Röntgen Radiation."

direction from the radiator in the secondary beam, passed through the apertures and entered electroscope A_2 through a thin paper and aluminium face.

The charging rod of each electroscope was connected to one terminal of a battery of 150 Leclanché cells, whose other terminal was earthed, so that the insulated wire and gold-leaf of each could be charged by means of the contact-maker, which momentarily connected the rod and wire, leaving the wire and gold-leaf charged and insulated.

The normal leak in each electroscope due to the ionisation of air within the electroscope case was of course a much more considerable fraction of the total leak during the period of X-ray production than in the previous experiments on secondary radiation, and the rate of ionisation due to all causes was so small that the saturation current between the gold-leaf and electroscope case would have been obtained by a small fraction of the potential gradient here used.

In the lead box the Röntgen-ray tube was situated so that the line joining the centre of its antikathode to the middle of the radiator R_1 was approximately perpendicular to the line joining the mid points of apertures C_1 and C_2, and no primary radiation passed through the aperture S_2.

All the rays passing through S_2 were then secondary rays from R_1 and from air, and these set up a tertiary radiation in R_2 and the air in the neighbourhood, some of which passed through the electroscopes set in position to indicate relative intensities of this tertiary radiation.

The secondary and tertiary radiations from air were of course small in comparison with the similar radiations from the large masses of carbon R_1 and R_2. A few simple experiments showed that the ionisation occurring in the electroscopes beyond the normal was due, as was expected, almost entirely to radiation proceeding from R_2 and set up by radiation from R_1, *i.e.*, to tertiary radiation.

To show the conclusiveness of the experiments, these will be described in detail.

The rates of deflection of the gold leaves in both electroscopes were first determined when the X-ray tube was not excited. These gave the effects of the normal ionisation taking place within the electroscope cases.

A discharge was passed through the X-ray tube for a definite time and the deflections were again observed; it was found that the rates of deflection were considerably increased by amounts depending on the direction of propagation of the primary beam incident on R_1 as well as on the intensity of the primary radiation. Dissimilarities in the construction of the electroscopes made these rates of deflection not accurately proportional to the intensities in

the two directions, but they were easily standardised by placing the X-ray tube in a position such that the primary beam was in a direction symmetrical with regard to the two directions, that is, making an angle of 45° with the vertical and horizontal tertiary beams studied.

It was soon seen that these increases in the rates of deflection were due almost entirely to the tertiary beams whose relative intensities it was desired to measure. By removing the radiator R_1, the rates of deflection became small again, showing that the effects of direct primary radiation through the screens, stray secondary radiation, tertiary radiation from air in the neighbourhood of R_2, etc., were small.

The radiation from R_1 was therefore the direct or indirect cause of the additional deflections.

By placing lead screens successively at apertures C_1 and C_2, and again observing the deflections during discharge when R_1 was in position, it was seen that the radiation directly or indirectly producing ionisation in the electroscopes passed through the two apertures, for the closing of these made the rates of deflection small again.

Finally, by removing the radiator R_2 it was proved that the deflections were not directly due to this secondary beam, but to a tertiary radiation proceeding from this radiator, for they became almost normal again; the tertiary radiation from air was of course small.

The X-ray tube was then turned about the axis of the secondary beam $R_1 R_2$, while the distance between the centres of the antikathode and radiator R_1 was unchanged. The radiator was so situated that the angle of incidence of the primary X-rays on it was unaltered when the primary beam KR_1 was turned through a right angle. This, though unnecessary in showing the relative variations in intensity of the two tertiary beams, made the results more convincing, for it showed independently the variations in these two beams due simply to rotation of the secondary beam.

As the X-ray tube was rotated in the manner indicated there was a considerable change in the intensities of the tertiary beams, one decreasing while the other increased. The horizontal tertiary beam reached a maximum in intensity when the primary beam was horizontal and a minimum when the primary was vertical; with the vertical tertiary the positions were reversed.

Some of the readings obtained are shown in Table I, p. 252.

Experiments 1, 2, 3 and 4 showed the deflections of the electroscopes under conditions referred to previously when the tertiary radiations, which it was desired to measure, were not set up. Thus the normal deflection due to causes other than the tertiary radiations (principally normal ionisation and ionisation produced by the very penetrating radiation from the bulb through

Table I.

Conditions of experiment.	Direction of primary beam.	Period of X-ray production.*	Readings and deflections of electroscope A_1 receiving horizontal tertiary beam.	Readings and deflections of electroscope A_2 receiving vertical tertiary beam.
1. Radiator R_1 absent	Horizontal	15 mins.	16·5, 18·5 } 2	50·2, 51·5 } 1·3
2. Lead screen at aperture C_1 ...	"	15 "	10·8, 12·6 } 1·8	43·5, 44·9 } 1·4
3. Lead screen at aperture C_2 ...	"	15 "	18·7, 10·4 } 1·7	55·7, 57·2 } 1·5
4. Radiator R_2 absent	"	15 "	18·5, 20·7 } 2·2	60·7, 62·55 } 1·85
5. Carbon radiators R_1 and R_2...	"	15 "	18·7, 26·9 } 8·2	51·4, 54·8 } 3·4
6. " " ...	Vertical	15 "	28·6, 32·3 } 3·7	56·6, 64·4 } 7·8
7. Carbon radiator R_1 and iron radiator R_2	"	15 "	11·7, 19·8 } 8·1	33·3, 41·2 } 7·9
8. " "	Horizontal	15 "	20, 28·1 } 8·1	39·7, 47·8 } 8·1

* The discharge was actually passed for only half a minute in each of the 15 minutes, in order to keep the tube more constant.

the lead screens and electroscope cases) were approximately 1·9 and 1·5 in the two electroscopes.

These had to be deducted from the deflexions in experiment 5, in which carbon radiators were used. When the primary beam was turned through a right angle, the deflections changed from 8·2 and 3·4 to 3·7 and 7·8 respectively. The corrections applied to the readings given in the second position (Experiment 6) were found as above to be 1·75 and 1·95. After correction the true readings were—

Direction of primary beam.	Deflection of electroscope A_1 receiving horizontal tertiary beam.	Deflection of electroscope A_2 receiving vertical tertiary beam.
Horizontal......	6·3	1·9
Vertical.........	1·95	5·85

Thus the horizontal intensity changed from 6·3 to 1·95, while the vertical intensity changed from 1·9 to 5·85. These numbers show the variation exceptionally well, as owing to slight irregular motion of the gold-leaves the readings could not be obtained with certainty to less than about 0·3 of a scale division. At times, however, the variations were very small and consequently accurate readings were obtainable.

The results are in striking contrast to those given by Experiments 7 and 8, in which the second radiator R_2 was of iron.

A number of experiments were made previous to those the results of which have been given. In these the sizes of apertures, the distance of the antikathode from the centre of radiator R_1 and other details were slightly different, but the possible error was not so small as in the later experiments. In every experiment however the same effect was clearly shown, the ratio of the intensities in the two principal directions being between 1 : 2·5 and 1 : 3·5.

The results of one of these are given below, as they show the deflections when the primary beam was horizontal, vertical and midway between the two. The preliminary experiments as shown above were not made, consequently the corrections were not accurately known. The numbers however show the kind of result that was obtained by a rough experiment without any special precautions.

Table II.

Conditions of experiment.	Direction of primary beam.	Period of X-ray production.	Readings and deflection of electroscope A_1 receiving horizontal tertiary radiation.	Readings and deflection of electroscope A_2 receiving vertical tertiary radiation.
Carbon radiators R_1 and R_2 ...	45° to vertical	15 mins.	$\left\{\begin{matrix}24{\cdot}4\\30{\cdot}9\end{matrix}\right\}$ 6·5	$\left.\begin{matrix}14{\cdot}6\\20{\cdot}5\end{matrix}\right\}$ 5·9
" " ...	Vertical	15 "	$\left\{\begin{matrix}31{\cdot}4\\34{\cdot}5\end{matrix}\right\}$ 3·1	$\left.\begin{matrix}21{\cdot}2\\28{\cdot}8\end{matrix}\right\}$ 7·6
" " ...	Horizontal	15 "	$\left\{\begin{matrix}35{\cdot}6\\45{\cdot}9\end{matrix}\right\}$ 10·3	$\left.\begin{matrix}29{\cdot}2\\31{\cdot}8\end{matrix}\right\}$ 2·6

These results were anticipated by a consideration of the theory of the production of secondary X-rays in carbon and other substances of low atomic weight.

When the direction of propagation of the primary beam was horizontal, the secondary radiation proceeding from the radiator R_1 in the direction R_1 R_2 was set up by the vertical components of electric displacement in the primary beam, consequently in the secondary beam the direction of electric displacement was vertical, and the intensity of tertiary radiation was therefore a maximum in a horizontal direction and zero in a vertical direction.*

As the beams studied were of considerable cross-section, the secondary here studied could not be completely polarised, for at any point there were superposed radiations proceeding in different directions from all the

* This reasoning is based on the assumption that there is perfect freedom of motion of the corpuscles in the atom. Even in light atoms there must be inter-corpuscular forces brought into play, consequently polarisation cannot be absolutely complete.

corpuscles in the radiator R_1. Nor, had there been complete polarisation of the secondary, could this have been detected by the study of tertiary beams of finite closs-section. Hence the ionisation in the electroscope receiving the vertical beam did not vanish, but reached a minimum.

The same reasoning applies to the case in which the direction of propagation of the primary beam was vertical, if we interchange the words vertical and horizontal. The variation in intensity of tertiary radiation in these experiments has been shown to be expressed approximately by the ratio 1 : 3. Thus the ratio of rates of ionisation in the two electroscopes changed from 6·3 : 1·9 to 1·95 : 5·85. Considering the obliquity of both secondary and tertiary rays in the beams studied, this is the order of result that might be expected if narrow pencils of radiation produced almost complete polarisation in the secondary.

To verify the dependence of the effect on the method of production of secondary X-radiation in substances of low atomic weight, and at the same time to obtain confirmation of the interpretation of these results, a metal plate of approximately the same area as the carbon was used as the second radiator R_2. Here again the choice of a substance was very limited, for while one was required that emitted a secondary radiation differing considerably from the primary (or in this case a tertiary differing from the secondary), it was impossible from the magnitude of the effects to sacrifice intensity. As in these experiments it was necessary to place the electroscopes at a distance of some centimetres from the radiator, the substance which emitted a radiation of such intensity and absorbability as to produce the maximum ionisation in the electroscopes at that distance was required. From a series of experiments it was found that iron was the most suitable.

The tertiary radiation from iron produced considerable ionisation in both electroscopes, but when the position of the tube was changed as before, no trace of the variation in intensities was detected. These results are shown by experiments 7 and 8 in Table I.

A number of observations were made in which, although there were changes in the intensity of primary radiation causing changes in the absolute values of the deflections, the ratio of these deflections remained constant within a few per cent. This is shown in Table III, p. 255.

This result, again, was what previous experiments on iron led one to expect.* It can be accounted for by considering the independence of motion of the corpuscles or electrons to disappear in the heavier atoms in which the systems are more complex. In these there is a much more

* See previous paper.

Table III.

Experiment.	Direction of primary beam.	Period of X-ray production.	Readings and deflections of electroscope A_1 receiving horizontal tertiary beam.	Readings and deflections of electroscope A_2 receiving vertical tertiary beam.	Ratio of deflections.
Carbon radiator R_1 and iron radiator R_2	Vertical	15 mins.	11·7, 19·8 } 8·1	33·3, 41·2 } 7·9	100 : 97·5
" "	Horizontal	15 "	20, 28·1 } 8·1	39·7, 47·8 } 8·1	100 : 100
" "	Vertical	15 "	19, 26·3 } 7·3	21·3, 28·3 } 7	100 : 96
" "	Horizontal	15 "	27·6, 35·8 } 8·2	29·9, 38·3 } 8·4	100 : 102·5
" "	Vertical	15 "	35·8, 43·1 } 7·3	40·3, 47·5 } 7·2	100 : 98·5

intimate connection between each corpuscle and its neighbours, and as a consequence each is subject to considerable forces during the period of passage of each pulse over a group of corpuscles in the neighbourhood, and the resultant acceleration is not in the direction of electric displacement in the secondary beam.* The difference in intensity of the tertiary in different directions hence disappears, while the pulse thickness in the tertiary beam becomes greater than in the secondary.

This experiment was perhaps the most conclusive proof of the interpretation of the results obtained with carbon, the essential point of difference in the two experiments being the substance of the radiator R_2. The order of magnitude of the ionisation produced was the same in the two cases, so that all other effects must have been equally prominent, yet the results were entirely different and may be fully explained from theoretical considerations of the processes taking place during the passage of Röntgen rays through different substances.

When the radiator R_1 was of iron and R_2 of carbon, the ionisation produced was too small to be measured. This is accounted for by the fact that the radiation from iron is much more readily absorbed, consequently only a very thin layer of carbon is penetrated by this radiation, and only a small mass is thus effective in producing tertiary radiation. A much greater fraction of the energy of secondary radiation is transformed into heat and less into energy of tertiary radiation.

We thus have evidence of a fairly complete polarisation in the secondary Röntgen radiation from carbon, and the theory of these radiations is further confirmed.

* The subject will be more fully dealt with in a paper on "Secondary Radiation."

Note on Heusler's Magnetic Alloy of Manganese, Aluminium, and Copper.

By ANDREW GRAY, F.R.S., Professor of Natural Philosophy in the University of Glasgow.

(Received December 15, 1905,—Read January 25, 1906.)

In 1903 Fr. Heusler published the discovery of an alloy consisting of manganese, aluminium, and copper, which, in spite of the fact that it contained none of the so-called magnetic metals, iron, nickel, or cobalt, possessed striking magnetic properties. Short accounts of work on the subject by Heusler and some other experimenters appeared,* but on the whole the discovery seems to have aroused comparatively little interest in this country before August, 1904, when R. A. Hadfield exhibited a specimen of the alloy at the Cambridge meeting of the British Association.

At the beginning of the winter session 1904 an attempt was made to obtain some of this alloy for the Physical Laboratory of Glasgow University, with the view of determining magnetic curves for the material, and of otherwise extending our knowledge of this interesting manganese bronze.

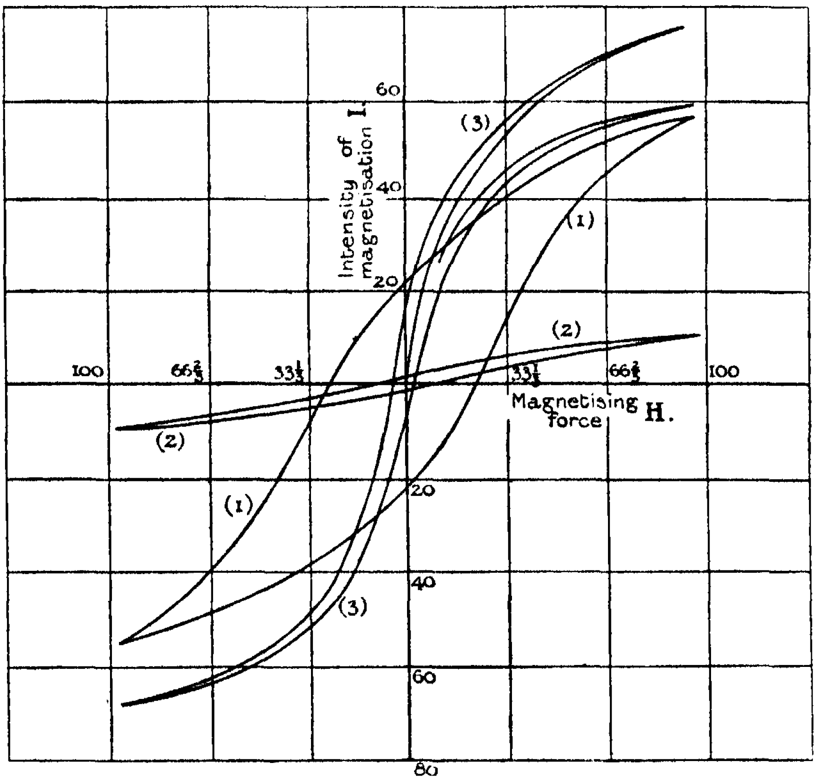
The publication of a paper by Fleming and Hadfield † to a certain extent supplied the information sought, but also served to emphasise the fact that various samples of this alloy possess very different magnetic properties. The maximum induction obtained by Fleming and Hadfield under a field of 200 C.G.S. units is less than one-half that obtained with a field of 150 in the case of one of Heusler's numerous samples. The form of the curves obtained by Fleming and Hadfield must also differ considerably from many of those indicated by the numbers given in Heusler's papers. Under these circumstances it has been decided to continue the work begun here, and to give now a short account of the work so far performed.

Dr. C. E. Fawsitt, of the Chemical Department of the University, became interested in the proposed research and attempted to make samples of the alloy. The apparatus at his disposal, however, did not admit of production of the alloy in anything but small quantities. The specimens obtained proved very retentive of magnetism, but were not suitable for the carrying out of quantitative work. In the meantime two rods of the alloy had been obtained from Herr Heusler by Dr. G. E. Allan, one of the research students in the Physical Laboratory. These rods were ground truly cylindrical on emery and

* *Cf.* 'Science Abstracts,' Nos. 622, 623, 636 (1904).

† 'Roy. Soc. Proc.,' June, 1905.

tested by the magnetometric method. The intensity of magnetisation (I) was calculated on the assumption that the effective lengths of the specimens were five-sixths of the actual lengths, and the magnetising field (H) was corrected by using the demagnetising factors for cylindrical rods given by Du Bois.



The first of the specimens contained about 26·5 per cent. manganese, 14·6 per cent. aluminium, and the remainder copper. This rod so far has only been tested in low magnetic fields; the intensity of magnetisation induced by a field of about 8 C.G.S. units was approximately 105.

The second rod obtained from Herr Heusler contained about 16 per cent. manganese, 8 per cent. aluminium, a little lead, and the remainder copper. After having been dressed, this rod was found to be practically non-magnetic (much less magnetic than is indicated by Curve 2 of the diagram). As it was said to be from the same pouring as another which showed well-marked magnetic qualities, it was conjectured that the heating and vibration to which the rod had been subjected during the dressing operations had destroyed the magnetic quality, and an attempt was made to restore the magnetic properties by thermal treatment. The rod was heated to 400° C. in

a furnace and allowed to cool slowly. After having been placed in a magnetic field it was found to retain a considerable amount of magnetism. It was next heated to 340° C. for about 20 minutes and allowed to cool, when it was found that the magnetic properties were much more pronounced. With the specimen in this state it was put through a cycle of magnetisation, and the results are shown in Curve 1 of the diagram.

It was now decided to try the effect of extremely low temperature upon the material. The specimen was immersed in liquid air, withdrawn, and put through a magnetic cycle as quickly as possible, the specimen warming up somewhat meanwhile. The effect produced was extremely slight, but was towards an increase in magnetic susceptibility.

An endeavour was next made to get the specimen into a better magnetic condition by heating to various temperatures,* but no improvement was obtained. Incidentally the critical temperature was found to be about 350° C.

An attempt was now made to destroy the magnetic quality of the material. Vigorous tapping at the temperature of the room was found to have no effect upon the residual magnetism. Previous tapping at the temperature of 100° C. had been found to produce a considerable reduction in the residual magnetism, but the original value was restored by again applying the magnetic field. It was thought that sudden cooling or "quenching" from above the critical temperature might permanently destroy the magnetic quality, and such was found to be the case. The specimen was heated to 400° C. in the furnace and then plunged vertically into cold water. Curve 2 of the diagram, which exhibits the results of a magnetic cycle carried through with the specimen after this treatment, shows the alloy to be in a comparatively non-magnetic condition. An examination of the specimen showed it to have several cracks distributed over its surface as a result of the quenching, and this probably affected the magnetic tests to a certain extent.

The effect produced by the temperature of liquid air upon the material in its quenched and, at ordinary temperatures, nearly non-magnetic condition was now investigated and found to be very remarkable. When tested at the temperature of liquid air the specimen was found to be more susceptible to magnetism than in its previous best condition, while it exhibited very much less hysteresis and retentiveness. Curve 3 of the diagram illustrates the new magnetic condition and shows how, moreover, the comparatively high susceptibility thus given to the alloy disappeared as the temperature rose. Curve 2 of the diagram was repeated after the temperature of the specimen had again become normal.

* Reference should be made to the extensive thermal experiments carried out by Heusler and his collaborators.

A microscopic examination of the material which we have been considering has been made. After many trials it was found that a solution of ammonium hydrate formed an efficient etching agent; the structure of the alloy, which had been revealed by polishing alone, was well brought out by this agent.

The work described here was carried out in the main by Dr. J. Muir and Mr. J. G. Gray, B.Sc., two of my assistants. We have now obtained a considerable quantity of the alloy in the form of rings and rods. These specimens have been cast for us by Messrs. Steven and Struthers, the well-known Glasgow engineers and brass-founders, and some of them are being turned into elongated ellipsoids of revolution. A continuation of the research on the lines indicated is in progress.

[*Addition, January* 15, 1906.—Curves analogous to 1, 2, 3 of the diagram have been obtained from specimens of nickel and steel. In the case of nickel the three curves differed only very slightly. On the steel specimen quenching had, of course, a considerable hardening effect; but the new magnetic condition was only slightly changed when the specimen was brought to the temperature of liquid air.]

On the Electric Resistance to the Motion of a Charged Conducting Sphere in Free Space or in a Field of Force.

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(Communicated by Professor A. E. H. Love, F.R.S. Received February 2,—Read March 2, 1905.)

In his investigations on the mode of decay of vibratory motion,* Prof. Love has demonstrated the great importance of considering the effect of the medium. In the particular case of a pendulum vibrating in air, it appears that the customary interpretation of the reaction of the medium on the pendulum, as an addition to the effective inertia along with a viscous term, is a good approximation under certain conditions. But in order to get an exact idea of what goes on, and justify the usual interpretation, it is necessary to examine in detail the motion of the medium, for it is only by this means that we can prescribe the conditions under which the usual interpretation is valid, and determine when this interpretation fails.

Turning to the case of electrical vibrations, it appears to me that Prof. Love's results have a very important bearing on the questions of electrical inertia and electrical damping. The present paper is an attempt to apply his method to some questions connected with the motion of an electrified spherical conductor.

In the paper referred to, a discussion is given of the vibration set up by a fixed conducting sphere when the electricity is initially distributed with a surface density proportional to the first zonal harmonic P_1. The total charge on the sphere is zero, and it is easy to verify that the resultant mechanical force on the sphere is nil.

If however we suppose that the sphere has a resultant charge, so that we have a uniform surface density superposed on the surface density proportional to P_1, then, since vibrations of zero order cannot be propagated, the determination of the vibrations of order unity is exactly the same as in the case discussed by Prof. Love. I find however that in this case there is a resultant mechanical force on the sphere in the direction of z, the axis of the harmonic P_1. Thus a mechanical force must be applied to the sphere in order to keep it at rest, and this force vanishes only when the vibrations have subsided. If the constraint is not applied the sphere must move.

The motion of a charged conductor in a uniform field of electrical force is

* 'Proc. Lond. Math. Soc.,' Ser. 2, vol. 2.

a matter of great importance. We shall begin by considering the following problem :—A perfectly conducting sphere of radius a, possessing a charge e is placed is a uniform field of electrical force F, and initially constraints exist which keep the sphere at rest with a uniform surface density. The constraints then cease to act, and we have to determine what ensues.

If z be the direction of F, the effect of F is (1) to make the sphere move in that direction, (2) to tend to establish an additional distribution on the surface proportional to the first zonal harmonic P_1. Vibrations of order unity thus become possible, and we shall confine our attention at first to the state of matters so long as it is permissible to neglect squares of the displacement; that is to say, if ζ is the displacement of the centre of the sphere, we shall neglect squares of ζ/a. As far as possible I shall follow Prof. Love's notation, taking the initial position of the centre of the sphere as a fixed origin of reference. I shall use x_0, y_0, z_0 for the co-ordinates of a point referred to this origin.

Initially we have at all points outside the sphere

$$(X, Y, Z) = e\left(\frac{x_0}{r_0^3}, \frac{y_0}{r_0^3}, \frac{z_0}{r_0^3}\right)+(0, 0, 1)\,F; \tag{1}$$
$$(\alpha, \beta, \gamma) = 0.$$

This state cannot continue. At a subsequent instant, when the centre of the sphere is at ζ along the z_0 axis, let the state of the medium outside the sphere be given by

$$(X, Y, Z) = e\left(\frac{x_0}{r_0^3}, \frac{y_0}{r_0^3}, \frac{z_0}{r_0^3}\right)+(0,0,1)\,F$$
$$+\mathrm{C}\left(\frac{\partial^2}{\partial x_0\partial z_0}, \frac{\partial^2}{\partial y_0\partial z_0}, -\frac{\partial^2}{\partial x_0^2}, -\frac{\partial^2}{\partial y_0^2}\right)\frac{\chi(\mathrm{C}t-r_0)}{r_0}; \tag{2}$$
$$(\alpha, \beta, \gamma) = \left(\frac{\partial^2}{\partial y_0\partial t}, -\frac{\partial^2}{\partial x_0\partial t}, 0\right)\frac{\chi(\mathrm{C}t-r_0)}{r_0}.$$

The conditions that must be satisfied at the surface $r_0 = \mathrm{C}t+a$, which separates the disturbed and undisturbed portions of the medium, require that

$$\chi = 0 \quad \text{and} \quad \chi' = 0 \quad \text{when } r_0 = \mathrm{C}t+a.$$

In order to apply the condition that the tangential component of electric force at the surface of the sphere should vanish, it is convenient to express the state of the medium with reference to a new fixed origin which at the instant coincides with the centre of the sphere. If x, y, z, r refer to this origin, and we may premise the result, which appears in the analysis, that χ and $e\zeta/\mathrm{C}$ are small quantities proportional to F, then we get approximately

$$(X, Y, Z) = e\left(\frac{x}{r^3}, \frac{y}{r^3}, \frac{z}{r^3}\right) - e\zeta\left(\frac{3xz}{r^5}, \frac{3yz}{r^5}, \frac{3z^2}{r^5} - \frac{1}{r^3}\right)$$
$$+ \mathrm{c}\left(\frac{\partial^2}{\partial x \partial z}, \frac{\partial^2}{\partial y \partial z}, -\frac{\partial^2}{\partial x^2}, -\frac{\partial^2}{\partial y^2}\right)\frac{\chi(\mathrm{c}t - r)}{r} + (0, 0, 1)\mathrm{F}; \quad (3)$$
$$(\alpha, \beta, \gamma) = \left(\frac{\partial^2}{\partial y \partial t}, -\frac{\partial^2}{\partial x \partial t}, 0\right)\frac{\chi(\mathrm{c}t - r)}{r}.$$

This may be written in the form

$$(X, Y, Z) = e\left(\frac{x}{r^3}, \frac{y}{r^3}, \frac{z}{r^3}\right) + (0, 0, 1)\left\{\mathrm{F} - \frac{\mathrm{c}}{r^3}\left(r^2\chi'' + r\chi' + \chi - \frac{e\zeta}{\mathrm{c}}\right)\right\}$$
$$+ \mathrm{c}\left(\frac{xz}{r^5}, \frac{yz}{r^5}, \frac{z^2}{r^5}\right)\left\{r^2\chi'' + 3r\chi' + 3\left(\chi - \frac{e\zeta}{\mathrm{c}}\right)\right\}; \quad (4)$$
$$(\alpha, \beta, \gamma) = \mathrm{c}\left(\frac{y}{r^3}, -\frac{x}{r^3}, 0\right)(r\chi'' + \chi').$$

From these we see that the tangential electric force vanishes at the surface of the sphere if

$$a^2\chi''(\mathrm{c}t - a) + a\chi'(\mathrm{c}t - a) + \chi(\mathrm{c}t - a) - \frac{e\zeta}{\mathrm{c}} = \frac{a^3\mathrm{F}}{\mathrm{c}}. \quad (5)$$

The surface density is given by

$$\sigma = \frac{1}{4\pi}\left\{\frac{e}{a^2} + \frac{\mathrm{c}P_1}{a^3}\left(a^2\chi'' + 3a\chi' + 3\chi - \frac{3e\zeta}{\mathrm{c}}\right)\right\}.$$

Or, in virtue of (5),

$$\sigma = \frac{1}{4\pi}\left\{\frac{e}{a^2} + \frac{\mathrm{c}P_1}{a^3}\left(\frac{3a^3\mathrm{F}}{\mathrm{c}} - 2a^2\chi''\right)\right\}. \quad (6)$$

The Z component of electric force

$$= \frac{eP_1}{a^2} + \mathrm{c}\frac{P_1^2}{a^3}\left(\frac{3a^3\mathrm{F}}{\mathrm{c}} - 2a^2\chi''\right). \quad (7)$$

It is easy to see that the resultant of all the mechanical forces when integrated over the sphere arises solely from the Z component of electric force.

The mechanical force in the direction of z

$$= \tfrac{1}{2}\int \sigma Z dS = e\mathrm{F} - \tfrac{2}{3}\frac{e\mathrm{c}}{a}\chi''(\mathrm{c}t - a). \quad (8)$$

Thus if m is the ordinary mass of the sphere, the equation of motion is

$$m\ddot{\zeta} = e\mathrm{F} - \tfrac{2}{3}\frac{e}{a}\mathrm{c}\chi''(\mathrm{c}t - a),$$

or

$$m\ddot{\zeta} + \tfrac{2}{3}\frac{e}{a}\mathrm{c}\chi''(\mathrm{c}t - a) = e\mathrm{F}. \quad (9)$$

Equations (5) and (9), along with the conditions $\chi(\mathrm{c}t-r)=0, \chi'(\mathrm{c}t-r)=0$, when $r=\mathrm{c}t+a$, and $\zeta=0$, $\dot{\zeta}=0$, when $t=0$, determine the motion.

It is of interest to note that equations (5) and (9) may be regarded as the equations of motion of a dynamical system with two degrees of freedom.

Putting $\chi=\frac{e}{\mathrm{C}}(\phi+\zeta)$, the equations may be written

$$m'\ddot{\phi}+m'\ddot{\zeta}+\frac{\mathrm{C}}{a}m'\dot{\phi}+\frac{\mathrm{C}}{a}m'\dot{\zeta}+\frac{\mathrm{C}^2}{a^2}m'\phi=\tfrac{2}{3}e\mathrm{F};$$

$$(m+m')\ddot{\zeta}+m'\ddot{\phi}=e\mathrm{F},$$

where
$$m'=\tfrac{2}{3}\frac{e^2}{a\mathrm{C}^2}.$$

Thus the kinetic energy is

$$\mathrm{T}=\tfrac{1}{2}m'(\dot{\phi}+\dot{\zeta})^2+\tfrac{1}{2}m\dot{\zeta}^2+\tfrac{1}{2}\frac{\mathrm{C}}{a}m'(\zeta\dot{\phi}-\phi\dot{\zeta}),$$

the dissipation function

$$\mathrm{F}=\tfrac{1}{2}\frac{\mathrm{C}}{a}m'\dot{\phi}(\dot{\phi}+\dot{\zeta}),$$

and the potential energy

$$\mathrm{V}=\tfrac{1}{2}\frac{\mathrm{C}^2}{a^2}m'\phi^2.$$

If we put $\xi=\mathrm{C}t-a$ equations (5) and (9) may be written

$$\frac{d^2\chi}{d\xi^2}+\frac{1}{a}\frac{d\chi}{d\xi}+\frac{1}{a^2}\left(\chi-\frac{e\zeta}{\mathrm{C}}\right)=\frac{a\mathrm{F}}{\mathrm{C}};\tag{10}$$

$$m\frac{d^2\zeta}{d\xi^2}+\tfrac{2}{3}\frac{e}{a\mathrm{C}}\frac{d^2\chi}{d\xi^2}=\frac{e}{\mathrm{C}^2}\mathrm{F},\tag{11}$$

with the conditions $\zeta=0$, $\dot{\zeta}=0$, when $t=0$ and $\chi(-a)=0$, $\chi'(-a)=0$.

The solution of (11) is

$$m\zeta+\tfrac{2}{3}\frac{e}{a\mathrm{C}}\chi=\tfrac{1}{2}\frac{e\mathrm{F}}{\mathrm{C}^2}\xi^2+\mathrm{B}\xi+\mathrm{D},$$

where B and D are constants of integration. Applying the initial conditions, we get

$$m\zeta+\tfrac{2}{3}\frac{e}{a\mathrm{C}}\chi=\tfrac{1}{2}\frac{e}{\mathrm{C}^2}\mathrm{F}(\xi+a)^2.\tag{12}$$

Substituting in (10), and putting $\frac{2}{3}\frac{e^2}{a\mathrm{C}^2}=m'$, we get

$$\frac{d^2\chi}{d\zeta^2}+\frac{1}{a}\frac{d\chi}{d\xi}+\frac{1}{a^2}\left(1+\frac{m'}{m}\right)\chi=\frac{a\mathrm{F}}{\mathrm{C}}\left\{1+\tfrac{3}{4}\frac{m'}{m}\frac{(\xi+a)^2}{a^2}\right\}.$$

The solution of this equation satisfying the initial conditions is

$$\chi=\mathrm{A}e^{-\frac{\xi+a}{2a}}\sin\left\{\left(3+\frac{4m'}{m}\right)^{\frac{1}{2}}\frac{\xi+a}{2a}+\epsilon\right\}+\mathrm{A}'(\xi+a)^2+\mathrm{B}'(\xi+a)+\mathrm{D}',\tag{13}$$

where

$$A' = \tfrac{3}{4}\,\frac{m'}{m+m'}\,\frac{a\mathrm{F}}{\mathrm{C}}, \qquad \mathrm{B}' = \tfrac{3}{2}\,\frac{mm'}{(m+m')^2}\,\frac{a^2\mathrm{F}}{\mathrm{C}},$$

$$\mathrm{D}' = \frac{m\,(2m^2+4mm'-m'^2)}{2\,(m+m')^3}\,\frac{a^3\mathrm{F}}{\mathrm{C}}, \qquad \mathrm{A}\sin\epsilon = -\mathrm{D}',$$

$$\left(3+\frac{4m'}{m}\right)^{\frac{1}{2}}\mathrm{A}\cos\epsilon = -(\mathrm{D}'+2a\mathrm{B}').$$

Hence

$$\chi(\mathrm{C}t-r) = \mathrm{A}\epsilon^{-(\mathrm{C}t-r+a)/2a}\sin\left\{\left(3+\frac{4m'}{m}\right)^{\frac{1}{2}}\frac{\mathrm{C}t-r+a}{2a}+\epsilon\right\}$$
$$+\mathrm{A}'\,(\mathrm{C}t-r+a)^2+\mathrm{B}'\,(\mathrm{C}t-r+a)+\mathrm{D}', \tag{14}$$

and

$$\xi = -\tfrac{2}{3}\,\frac{e}{ma\mathrm{C}}\,\mathrm{A}\epsilon^{-\mathrm{C}t/2a}\sin\left\{\left(3+\frac{4m'}{m}\right)^{\frac{1}{2}}\frac{\mathrm{C}t}{2a}+\epsilon\right\}$$
$$+\tfrac{1}{2}\,\frac{e\mathrm{F}}{(m+m')}\,t^2+\frac{e\mathrm{F}m'}{(m+m')^2}\,\frac{at}{\mathrm{C}}-\tfrac{1}{3}\,\frac{e\mathrm{F}\,(2m^2+4mm'-m'^2)}{(m+m')^3}\,\frac{a^2}{\mathrm{C}^2}. \tag{15}$$

These results hold as long as ζ/a is small. We may observe that the initial displacement expressed by the damped harmonic part of the motion is equal and opposite to the displacement expressed by the non-periodic portion. After one complete vibration the amplitude of the vibratory part is $\epsilon^{-2\pi/(3+4m'/m)^{\frac{1}{2}}}$ of its initial value. Thus if the displacement at time t is small, the vibratory part of the motion may have practically become insignificant before the equations become invalid. Since the decay is exponential, this will be secured even when $\mathrm{C}t/a$ is only moderately great, while the condition that ζ/a is small can be secured by making F small.

In these circumstances the displacement of the sphere is adequately represented by

$$\zeta = \tfrac{1}{2}\,\frac{e\mathrm{F}}{(m+m')}\,t^2+\frac{e\mathrm{F}m'}{(m+m')^2}\,\frac{a}{\mathrm{C}}\,t-\tfrac{1}{3}\,\frac{e\mathrm{F}\,(2m^2+4mm'-m')^2}{(m+m')^3}\,\frac{a^2}{\mathrm{C}^2}, \tag{16}$$

and

$$\chi\,(\mathrm{C}t-r) = \tfrac{3}{4}\,\frac{m'}{m+m'}\,\frac{a\mathrm{F}}{\mathrm{C}}\,(\mathrm{C}t-r+a)^2-\tfrac{3}{2}\,\frac{mm'}{(m+m')^2}\,\frac{a^2\mathrm{F}}{\mathrm{C}}\,(\mathrm{C}t-r+a)$$
$$+\frac{m\,(2m^2+4mm'-m'^2)}{2\,(m+m')^3}\,\frac{a^3\mathrm{F}}{\mathrm{C}}, \tag{17}$$

within a certain region.

We may readily verify that the state of the field within this region is given by

$$(\mathrm{X},\mathrm{Y},\mathrm{Z}) = e\left(\frac{x}{r^3},\,\frac{y}{r^3},\,\frac{z}{r^3}\right)+(0,0,1)\left\{\mathrm{F}-\frac{\mathrm{C}}{r^3}(\mathrm{L}+\mathrm{A}'r^2)\right\}$$
$$+\mathrm{C}\left\{\frac{xz}{r^5},\,\frac{yz}{r^5},\,\frac{z^2}{r^5}\right\}\{3\mathrm{L}+\mathrm{A}'r^2\}; \qquad (18)^*$$

* At greater distances the damped harmonic train would have to be retained were the wave-train not limited.

$$(\alpha, \beta, \gamma) = \left(\frac{y}{r^3}, -\frac{x}{r^3}, 0\right)\frac{e\dot{\zeta}}{\mathrm{C}},$$

where

$$\mathrm{A}' = \tfrac{3}{4}\frac{m'}{m+m'}\frac{a\mathrm{F}}{\mathrm{C}}, \qquad \mathrm{L} = \frac{a^3\mathrm{F}}{\mathrm{C}} - a^2\mathrm{A}',$$

the origin being the centre of the sphere at time t. The surface density is

$$\sigma = \frac{1}{4\pi}\left\{\frac{e}{a^2} + 3\mathrm{P}_1\frac{2m+m'}{2(m+m')}\mathrm{F}\right\}. \tag{19}$$

Before discussing these results, and in case exception may be taken to the artificial nature of the initial state, I shall consider the following case. The charged sphere is placed in a uniform field and held at rest until the distribution of electricity is that appropriate to a uniform external applied field. The sphere is then let go, and we have to determine the subsequent state. The initial state is thus given by

$$(\mathrm{X}, \mathrm{Y}, \mathrm{Z}) = e\left(\frac{x_0}{r_0^3}, \frac{y_0}{r_0^3}, \frac{z_0}{r_0^3}\right) + (0, 0, 1)\mathrm{F}$$
$$+\mathrm{F}a^3\left(\frac{3x_0z_0}{r_0^5}, \frac{3y_0z_0}{r_0^5}, \frac{3z_0^2}{r_0^5} - \frac{1}{r_0^3}\right);$$
$$(\alpha, \beta, \gamma) = 0.$$

If we proceed in the same manner as before, it is fairly obvious that the differential equations for ζ and χ are the same as before, viz., (10) and (11). The initial conditions are now $\zeta = 0$, $\dot{\zeta} = 0$, when $t = 0$ and $\chi(-a) = \mathrm{F}a^3/\mathrm{C}$, $\chi'(-a) = 0$.

Under the same conditions as before, I find that when the vibratory part has become negligible,

$$\zeta = \tfrac{1}{2}\frac{e\mathrm{F}}{(m+m')}t^2 + \frac{e\mathrm{F}m'}{(m+m')^2}\frac{a}{\mathrm{C}}t - \tfrac{1}{3}\frac{e\mathrm{F}(2m^2+4mm'-m'^2)}{(m+m')^3}\frac{a^2}{\mathrm{C}^2}$$
$$+\tfrac{2}{3}\frac{e\mathrm{F}}{(m+m')}\frac{a^2}{\mathrm{C}^2}, \tag{20}$$

and

$$\chi(\mathrm{C}t-r) = \tfrac{3}{4}\frac{m'}{(m+m')}\frac{a\mathrm{F}}{\mathrm{C}}(\mathrm{C}t-r+a)^2 - \tfrac{3}{2}\frac{mm'}{(m+m')^2}\frac{a^2\mathrm{F}}{\mathrm{C}}(\mathrm{C}t-r+a)$$
$$+\frac{m(2m^2+4mm'-m'^2)}{2(m+m')^3}\frac{a^3\mathrm{F}}{\mathrm{C}} + \frac{m'}{(m+m')}\frac{a^3\mathrm{F}}{\mathrm{C}}. \tag{21}$$

The equations (20) and (21) differ from (16) and (17) only in the constant terms. I have also verified that (20) and (21) give the same state of the medium (18) and the same surface density (19).

Equations (16) and (20) both give

$$\dot{\zeta} = \frac{e\mathrm{F}}{(m+m')}t + \frac{e\mathrm{F}m'}{(m+m')^2}\frac{a}{\mathrm{C}}; \qquad \ddot{\zeta} = \frac{e\mathrm{F}}{m+m'}.$$

The sphere thus arrives at the position given by (16) or (20), as if the equation of motion had been

$$(m+m')\ddot{\zeta} = e\mathrm{F}, \tag{22}$$

and the initial velocity had been $\dfrac{e\mathrm{F}m'}{(m+m')^2}\dfrac{a}{\mathrm{C}}$, and the initial position $-\frac{1}{3}\dfrac{e\mathrm{F}\,(2m^2+4mm'-m'^2)}{(m+m')^2}\dfrac{a^2}{\mathrm{C}^2}$ in the case (16), and $\dfrac{e\mathrm{F}m'^2}{(m+m')^3}\dfrac{a^2}{\mathrm{C}^2}$ in the case (20)

This seems to me the only interpretation which can be put on the equations. It thus appears that the effect of the medium is to contribute m', *i.e.*, $\dfrac{2}{3}\dfrac{e^2}{a\mathrm{C}^2}$ to the inertia in the equation of motion, and so far this agrees with the usual result. No other term occurs in the equation of motion (22), and the other effects are a permanent contribution to the velocity and a permanent contribution to the displacement. It thus appears that when the state represented by (16)—(19) is reached no damping action of the medium takes place. This is supported by equations (18) and (19), which show that a surface density independent of the time has been established for an origin moving with the sphere, and that the electric field in the immediate vicinity of the sphere is also independent of the time. Since $\dot{\zeta}$ increases uniformly with time, the magnetic field varies. The only difference as time goes on is that the region throughout which (18) is applicable becomes greater.

This result is remarkable, and requires further examination, as it may appear to conflict with Larmor's calculation* of the rate at which energy crosses a surface at some distance from a small charged system under constant acceleration, for his result suggests continual damping.

There are however several points to be remembered.

The expressions given by Larmor for the electric and magnetic forces apply only at great distances from the origin, and require modification in the vicinity of the small system, *e.g.*, an electron which has a definite size.†

The rate at which energy travels outwards is calculated over a surface of large radius. It must include the rate at which the portion of energy $\frac{1}{2}m'\dot{\zeta}^2$ already established is redistributed throughout space. Whether this can be regarded as lost is doubtful. Larmor points out that the flow of energy in the vicinity of the small system would be different.

The rate is calculated on the supposition that the only forces are those due

* 'Æther and Matter,' p. 226, *et. seq.*

† When the sphere has a definite radius the surface at any instant will cut the space enclosed by its surface at a short interval of time previously, and thus an expansion in powers of ζ/a can be obtained. If the charge is concentrated at a mere point, this expansion fails, and some new procedure would have to be adopted.

to the motion of the system. The rate would be modified if, as in the present case, any other field of force exists.

Thus the rate calculated by Larmor cannot here be applied to give the reaction on the small system at any instant.* There is no conflict, for the conditions assumed are not the same. If the only field is that due to the motion, the radiated energy must be supplied at the expense of the energy of the moving system, and this means damping of the motion. But when in addition there is an external field producing the motion, the external field may supply the radiated energy, and the damping action is masked. The cases are analogous to those of an ordinary vibrating mechanical system with dissipation under the influence of (1) no forces, and (2) given applied forces.

The equation $(m+m')\ddot{\zeta} = e\mathrm{F}$ is not to be taken as meaning that there is no transference of energy across the surface of the sphere, for we have seen that the constants of integration have to be given certain values which indicate an apparent initial velocity and initial displacement. A reference to the dynamical specification on p. 263 shows that the external field does work in altering the co-ordinate ϕ as well as in altering the co-ordinate ζ.

The results may also be regarded as not inconsistent with those obtained by Lorentz and others. Lorentz† shows that the damping effect is represented by a term proportional to $\dddot{\zeta}$ in the equation of motion, and here when the vibrations have subsided $\dddot{\zeta}$ is zero.

By eliminating χ between (5) and (9) we get an equation for ζ which is valid throughout the whole time considered. The equation is—

$$(m+m')\ddot{\zeta} + \frac{ma}{\mathrm{C}}\dddot{\zeta} + \frac{ma^2}{\mathrm{C}^2}\ddddot{\zeta} = e\mathrm{F}.$$

This might be regarded as the equation of motion, but since the solution involves four arbitrary constants, we cannot complete their determination without a knowledge of the medium.

We thus conclude that if the vibratory part becomes negligible, while ζ/a is still small, the equation of motion is

$$(m+m')\ddot{\zeta} = e\mathrm{F},$$

and the position at time t is the solution of this, as if the sphere had started with a certain initial velocity and a certain initial displacement from the original centre of the sphere.

In connection with the question of electrical inertia it is of interest to

* *Cf.* Sommerfeld, 'Gött. Nachrichten,' 1904, vol. 5, p. 369.

† 'Théorie Electromagnétique,' p. 124.

consider what happens if m is zero. We shall examine the case where the sphere is held fixed until the surface density appropriate to a uniform field is established, and the sphere then released.

Equations (10) and (11) now become

$$\frac{d^2\chi}{d\xi^2}+\frac{1}{a}\frac{d\chi}{d\xi}+\frac{1}{a^2}\left(\chi-\frac{e\zeta}{c}\right)=\frac{a\mathrm{F}}{c}. \tag{23}$$

and

$$\tfrac{2}{3}\frac{e}{ac}\frac{d^2\chi}{d\xi^2}=\frac{e}{c^2}\mathrm{F}. \tag{24}$$

The initial conditions are $\zeta=0$, $\dot{\zeta}=0$, when $t=0$, $\chi(-a)=a^3\mathrm{F}/c$, $\chi'(-a)=0$.

The solution of (24) is

$$\chi=\tfrac{3}{4}\frac{a}{c}\mathrm{F}(\xi+a)^2+\frac{a^3\mathrm{F}}{c}.$$

Hence
$$\chi(ct-r)=\tfrac{3}{4}\frac{a}{c}\mathrm{F}(ct-r+a)^2+\frac{a^3\mathrm{F}}{c}.$$

Substituting in (23) we get

$$\zeta=\tfrac{1}{2}\frac{e\mathrm{F}}{m'}t^2+\frac{a}{c}\frac{e\mathrm{F}}{m'}t+\frac{e\mathrm{F}}{m'}\frac{a^2}{c^2},\quad\text{where}\quad m'=\tfrac{2}{3}\frac{e^2}{ac^2}.$$

It appears that no vibratory part is set up, but the expression for ζ does not satisfy the conditions $\zeta=0$ $\dot{\zeta}=0$ when $t=0$, for we cannot make a zero without invalidating the whole thing. We must thus regard the equations as failing when $m=0$.

The question naturally arises whether any of the conclusions arrived at would be completely invalidated by retaining squares of ζ/a. If we retained squares and higher powers of ζ/a, it is obvious that we should have to assume vibrations of the second and higher orders. In the initial stages of the motion the terms introduced by the vibrations of the nth order will be proportional to F^n. Thus by taking F small enough we can make the first order terms already investigated as good an approximation as we please. Further, the vibrations of higher order die out more rapidly the higher the order. We may therefore conclude that by taking F small, the additional terms introduced will in the initial stages be small in comparison with the first order terms, and hence the conclusions will not be substantially modified.

When the state represented by equations (18), (19), (22) is reached, a constant surface density is established, and an electric field which in the vicinity of the sphere is independent of the time. The continuance of the motion cannot, as far as I can see, set up any new damped harmonic vibration of the first order, and equations (18), (19), (22) continue to represent the

state with respect to the centre of the sphere at the instant considered, the only difference as time goes on being that the space throughout which (18) is applicable increases. We may indicate the way in which this process must fail. Vibrations of higher orders must arise, and the velocity of the sphere becomes so great that the interval of time τ from t to $t+\tau$, during which ζ/a (ζ being the displacement of the sphere from a fixed origin coinciding with the centre of the sphere at time t) is small, becomes less and less, so that the vibrations have not time to become insignificant. Whether, with the vibrations of higher order, as in the case of the first order vibrations, a state is reached when no further vibration of a particular order can be set up, is a question for further investigation.

If at a time t, while equations (14) and (15) are still valid, the external field is supposed to cease, new vibrations of the first order are set up, and the system settles down to a new state.

The procedure is much the same as before. Taking the centre of the sphere at time t_1 as a fixed origin of reference, and that instant as a new origin of time, we assume quantities θ for the displacement of the sphere and $\psi\,(Ct-r)$ for the vibrations. Neglecting squares of θ/a, the equations are

$$a^2\psi''\,(\mathrm{c}t-a)+a\psi'\,(\mathrm{c}t-a)+\psi\,(\mathrm{c}t-a)-e\,(\theta/\mathrm{c}) = 0\,;$$
$$m\ddot{\theta}+\tfrac{2}{3}\,(e\mathrm{c}/a)\,\psi''\,(\mathrm{c}t-a) = 0,$$

which hold at the surface of the sphere with the initial conditions

$$\psi'\,(-a) = \chi'\,(\mathrm{c}t_1), \qquad \psi\,(-a) = \chi\,(\mathrm{c}t_1)\,(e\zeta_{t_1})/\mathrm{c},$$

which hold at the surface of discontinuity, and

$$\theta = 0, \qquad \dot{\theta} = \dot{\zeta}_{t_1},$$

the values being determined from (14) and (15).

The solution of these equations is

$$\psi\,(\mathrm{c}t-r) = \mathrm{A}''e^{(\mathrm{c}t-r+a)/2a}\sin\left\{\left(3+\frac{4m'}{m}\right)^{\frac{1}{2}}\frac{\mathrm{c}t-r+a}{2a}+\epsilon''\right\}$$
$$+\frac{e}{(m+m')\,\mathrm{c}}\,\mathrm{B}\,(\mathrm{c}t-r+a)+\frac{e}{(m+m')\,\mathrm{c}}\left\{\mathrm{D}-\frac{am'}{(m+m')}\,\mathrm{B}\right\},$$

referred to the centre of the sphere at the instant considered, and t being now reckoned from the instant t_1.

The displacement of the sphere is given by

$$\theta = -\tfrac{2}{3}\,\frac{e}{am\mathrm{c}}\,\mathrm{A}''\,e^{-\mathrm{c}t/2a}\sin\left\{\left(3+\frac{4m'}{m}\right)^{\frac{1}{2}}\left(\frac{\mathrm{c}t}{2a}+\epsilon''\right)\right\}$$
$$+\frac{\mathrm{c}t\mathrm{B}}{(m+m')}+\frac{\mathrm{D}}{(m+m')}+\frac{am'}{(m+m')^2}\,\mathrm{B}.$$

The constants are given by

$$D = \left\{\chi(ct_1) - \frac{e}{c}\zeta_{t_1}\right\}\tfrac{2}{3}\frac{e}{ac} = -(m+m')\,\zeta_{t_1} + m\zeta_{t_1} + \tfrac{2}{3}\frac{e}{ac}\chi(ct_1);$$

$$B = m\left(\frac{d\zeta}{d\xi}\right)_{t_1} + \tfrac{2}{3}\frac{e}{ac}\left(\frac{d\chi}{d\xi}\right)_{t_1}.$$

$$A''\sin\epsilon'' = \chi(ct_1) - \frac{e\zeta_{t_1}}{c} - \frac{e}{(m+m')\,c}\left\{D - \frac{am}{(m+m')}B\right\},$$

$$-A''\sin\epsilon'' + \left(3 + 4\frac{m'}{m}\right)^{\frac{1}{2}} A''\cos\epsilon'' = 2a\left(\frac{d\chi}{d\xi}\right)_{t_1} - \frac{2ac}{(m+m')\,c}B.$$

If this new set of vibrations become negligible while θ/a is still small, we get as before

$$\psi(ct-r) = \frac{eB}{(m+m')\,c}(ct-r+a) + \frac{e}{(m+m')\,c}\left\{D - \frac{am}{(m+m')^2}B\right\},$$

and
$$\theta = \frac{B}{(m+m')}ct + \frac{D}{(m+m')} + \frac{am'B}{(m+m')^2}.$$

It is readily verified that these give for the field

$$(X, Y, Z) = e\left(\frac{x}{r^3}, \frac{y}{r^3}, \frac{z}{r^3}\right), \qquad (\alpha, \beta, \gamma) = e\left(\frac{y}{r^3}, -\frac{x}{r^3}, 0\right)\frac{\dot\theta}{c},$$

applicable within a certain region.

This agrees with the usual result that when a constant velocity has been established, the electric field given by $e(x/r^3, y/r^3, z/r^3)$, is carried with the sphere, while the magnetic field is that due to an element of current $e\dot\theta/c$.

We may determine B and D from equation (12), which holds whether or not the vibrations set up in starting the system have subsided at the instant t_1, when F is supposed to cease.

We thus obtain

$$\theta = \frac{eFt_1}{(m+m')}t - \zeta_{t_1} + \tfrac{1}{2}\frac{eF}{(m+m')}t_1^2 + \frac{a}{c}\frac{m'}{(m+m')^2}eFt_1.$$

Hence the velocity finally established is $eFt_1/(m+m')$, which is the velocity acquired by the system having inertia $m + m'$ acted on by the force eF for a time t_1, so that the apparent contribution to the initial velocity (see p. 266) by the first vibrations is exactly destroyed by the second vibrations. Again writing the above equation in the form

$$\theta + \zeta_{t_1} = \frac{eFt_1}{(m+m')}t + \tfrac{1}{2}\frac{eF}{(m+m')}t_1^2 + \frac{a}{c}\frac{m'}{(m+m')^2}eFt_1,$$

the apparent contribution to the initial displacement produced by the first vibrations is destroyed by the second vibrations. The contribution

$\frac{1}{2}\frac{eF}{(m+m')}t_1^2$ is the displacement due to the force eF acting on $(m+m')$ for a time t_1. The contribution on account of the velocity $\frac{a}{c}\frac{m_1}{(m+m')^2}eF$ for a time t_1, could not of course be expected to disappear. It is the only permanent effect of the medium on the sphere which could not be accounted for by the ordinary dynamics of a particle of mass $(m+m')$ acted on by a force eF for a time t_1, and then allowed to go on. Further, there is no loss of energy, for the velocity established is $eFt_1/(m+m')$, and the energy of the system is $\frac{1}{2}e^2F^2t_1^2/(m+m')$, *i.e.*, $eF \cdot \frac{1}{2}eFt_1^2/(m+m')$, and is thus the work done by the force eF acting on the particle of mass $(m+m')$ for a time t_1.

The problem is thus brought into exceedingly close relationship with ordinary dynamics.*

The satisfaction of the energy condition emphasises what I have already said about the rate at which energy crosses a boundary far away from the sphere. It must not all be regarded as energy lost to the system, but part of it must be taken as indicating how the portion of energy $\frac{1}{2}m'v^2$ is redistributed throughout space as time goes on.

If the field applied to the charge is of a periodic character, the sphere may be supposed never to move far from its original position. Thus if ζ/a is always small, the first order terms are adequate for an indefinitely great time. We shall suppose that at time $t=0$ the periodic force $F\cos nt$ begins to act.

Referring to (10) and (11), the equations for χ and ζ are readily seen to be

$$\frac{d^2\chi}{d\xi^2}+\frac{1}{a}\frac{d\chi}{d\xi}+\frac{1}{a^2}\left(\chi-\frac{e\zeta}{c}\right)=\frac{aF}{c}\cos nt; \tag{25}$$

$$m\frac{d^2\zeta}{d\xi^2}+\frac{2}{3}\frac{e}{ac}\frac{d^2\chi}{d\zeta^2}=\frac{e}{c^2}F\cos nt, \tag{26}$$

with the conditions $\zeta=0$, $\dot{\zeta}=0$, when $t=0$ and $\chi(-a)=0$, $\chi'(-a)=0$.

The solution of (26) is

$$m\zeta+\frac{2}{3}\frac{e}{ac}\chi=-\frac{e}{n^2}F\cos nt+\frac{eF}{n^2}.$$

Hence substituting in (25) we get

$$\frac{d^2\chi}{d\xi^2}+\frac{1}{a}\frac{d\chi}{d\xi}+\frac{1}{a^2}\left(1+\frac{m'}{m}\right)\chi=\left(\frac{a}{c}-\frac{e^2}{a^2cmn^2}\right)F\cos nt+\frac{e^2F}{mn^2a^2c}.$$

* In agreement with this we may also observe that the dissipation function, p. 263, which may be written $\frac{1}{2}c\dot{\psi}(\dot{\psi}-e\dot{\theta}/c)$, now vanishes, since the equations give $\dot{\psi}-e\dot{\theta}/c=0$.

As the solution of this we get

$$\chi(\mathrm{c}t-r) = \mathrm{A}e^{-(\mathrm{c}t-r+a)/2a}\sin\left\{\left(3+4\frac{m'}{m}\right)^{\frac{1}{2}}\frac{\mathrm{c}t-r+a}{2a}+\epsilon\right\}+\frac{e^2\mathrm{F}}{(m+m')n^2\mathrm{c}}$$

$$+\left(\frac{a}{\mathrm{c}}-\frac{e^2}{a^2mn^2\mathrm{c}}\right)\mathrm{F}\frac{\left\{\dfrac{(m+m')}{a^2m}-\dfrac{n^2}{\mathrm{c}^2}\right\}\cos\dfrac{n(\mathrm{c}t-r+a)}{\mathrm{c}}-\dfrac{n}{a\mathrm{c}}\sin\dfrac{n(\mathrm{c}t-r+a)}{\mathrm{c}}}{\left(\dfrac{m+m'}{a^2m}-\dfrac{n^2}{\mathrm{c}^2}\right)^2+\dfrac{1}{a^2}\dfrac{n^2}{\mathrm{c}^2}},$$

where A and ϵ are determined by the initial conditions $\chi(-a)=0, \chi'(-a)=0$.

Hence we get

$$\zeta = -\tfrac{2}{3}\frac{e}{am}\mathrm{A}e^{-\mathrm{c}t/2a}\sin\left\{\left(3+\frac{4m'}{m}\right)^{\frac{1}{2}}\frac{\mathrm{c}t}{2a}+\epsilon\right\}+\frac{e\mathrm{F}}{(m+m')n^2}-\frac{e\mathrm{F}}{mn^2}\mathrm{F}\cos nt$$

$$-\frac{e}{m}\left(\frac{2}{3\mathrm{c}^2}-\frac{m'}{ma^2n^2}\right)\mathrm{F}\frac{\left\{\dfrac{(m+m')}{a^2m}-\dfrac{n^2}{\mathrm{c}^2}\right\}\cos nt-\dfrac{n}{a\mathrm{c}}\sin nt}{\left\{\dfrac{(m+m')}{a^2m}-\dfrac{n^2}{\mathrm{c}^2}\right\}^2+\dfrac{1}{a^2}\dfrac{n^2}{\mathrm{c}^2}}. \quad (27)$$

The damped harmonic part rapidly becomes negligible, and we then have

$$\zeta = \frac{e}{(m+m')n^2}\mathrm{F}-\frac{e}{mn^2}\mathrm{F}\cos nt$$

$$-\frac{e}{m}\left(\frac{2}{3\mathrm{c}^2}-\frac{m'}{ma^2n^2}\right)\mathrm{F}\frac{\left\{\dfrac{(m+m')}{a^2m}-\dfrac{n^2}{\mathrm{c}^2}\right\}\cos nt-\dfrac{n}{a\mathrm{c}}\sin nt}{\left\{\dfrac{(m+m')}{a^2m}-\dfrac{n^2}{\mathrm{c}^2}\right\}^2+\dfrac{n^2}{a^2\mathrm{c}^2}}. \quad (28)$$

I find as in the former examples that the initial conditions cannot be satisfied if $m=0$, as no damped harmonic part is set up.

It is possible to interpret (28) as a solution of the equation

$$\mathrm{M}\ddot{\zeta}+k\dddot{\zeta} = e\mathrm{F}\cos nt,$$

where, with some algebraic reduction, it is found that,

$$\mathrm{M} = \frac{(m+m')-\frac{1}{3}(m+m')\dfrac{a^2n^2}{\mathrm{c}^2}-\frac{1}{3}m\dfrac{a^4n^4}{\mathrm{c}^4}}{1+\frac{1}{3}\dfrac{a^2n^2}{\mathrm{c}^2}+\frac{1}{9}\dfrac{a^4n^4}{\mathrm{c}^4}}, \qquad k = \frac{\left(m'-\frac{2}{3}m\dfrac{a^2n^2}{\mathrm{c}^2}\right)\dfrac{a}{\mathrm{c}}}{1+\frac{1}{3}\dfrac{a^2n^2}{\mathrm{c}^2}+\frac{1}{9}\dfrac{a^4n^4}{\mathrm{c}^4}}.$$

We cannot make a zero, since the process assumes that ζ/a is small, but if the frequency is so small that a^2n^2/c^2 may be neglected, we get approximately $\mathrm{M} = m+m'$, $k = \frac{2}{3}(e^2/\mathrm{c}^3)$. This agrees with Lorentz's* result for an electron. It also agrees with the first term in M. Abraham's† result for a Heaviside ellipsoid when the velocity is small.

* 'Ency. d. Math. Wiss.,' vol. 5, Part 2, p. 190.

† 'Electrician,' pp. 868, 869, September 16, 1904.

By eliminating χ between (25) and (26) we get

$$(m+m')\ddot{\zeta}+\frac{ma}{\mathrm{C}}\dddot{\zeta}+m\frac{a^2}{\mathrm{C}^2}\ddddot{\zeta}=e\mathrm{F}\left\{\cos nt-\frac{na}{\mathrm{C}}\sin nt-\tfrac{1}{3}\frac{n^2a^2}{\mathrm{C}^2}\cos nt\right\},$$

applicable from the initial instant. For the complete determination of the arbitrary constants the initial values of ζ and $\dot{\zeta}$ are not sufficient.

These examples seem to me to emphasise the necessity of considering the equations for the sphere and the medium side by side, for even when it is possible to get a differential equation for the displacement of the sphere, it appears that we cannot determine the constants of integration without a knowledge of the state of the medium.

I should like to acknowledge my great obligation to Prof. Love for his kindness in criticising this paper. On his suggestion some portion of it has been rewritten, and the results thereby represented in a more satisfactory manner.

Galvanic Cells Produced by the Action of Light.—The Chemical Statics and Dynamics of Reversible and Irreversible Systems under the Influence of Light. (*Second Communication.*)

By Meyer Wilderman, Ph.D., B.Sc. (Oxon.)

(Communicated by Dr. Ludwig Mond, F.R.S. Received June 27, 1905,—
Read January 25, 1906.)

(Abstract.)

The following is a summary of the different subjects dealt with in the paper.

(1) Further evidence is given that velocity of chemical reaction and chemical equilibrium in homogeneous systems follow under the action of light the laws of mass action.

(2) Experimental proof that the E.M.F. produced by light in the different systems consists of two E.M.F.'s, viz.: one created by light at a constant temperature due to the variation of chemical potential, and a thermo-E.M.F. simultaneously produced by the heating effect of the light, and due to the variation of the chemical potential with temperature (quantitative separation of the total E.M.F. into the two E.M.F.'s, and determination of the value of each E.M.F. in the different systems).

(3) Experimental proof that the rays of all wave-lengths act both "chemically" and as "heat rays," only in different degrees.

(4) Dealing with the experiments of Becquerel and Minchin, it is shown that phenomena observed by Becquerel and Minchin are not surface phenomena, but that their combination forms inconstant galvanic cells under the action of light. Experimental proof. Polarisation. (Influence of composition of the heterogeneous system upon its properties, as constant or inconstant galvanic cells. The course of the parts of the curves giving the induction and deduction periods in constant and inconstant cells.)

(5) On the nature of the chemical processes in galvanic cells created by light.

(6) The method of investigation. The general arrangements of the experiments. The arrangements of a constant acetylene and arc light, of the photometer, the quartz vessel, the preparation of the plates, the bath, arrangements for calibration of the obtained results in standard units for photographing the effect of light upon different systems, etc.

(7) On the E.M.F.'s in the dark, gas batteries, etc.

(8) Chemical statics and dynamics of constant cells reversible in respect of

the cation (*e.g.*, Ag plates in NO_3Ag solution). The composition of such a system. The reactions going on in such a system. Proof that this system is reversible, that its composition remains constant under the action of the current, that the galvanic cells are *sui generis*. The relationship to Gibb's rule of phases.

(9) Experimental results obtained with constant cells reversible in respect of the cation. The constant deflections in light. The course of the deduction and induction periods. The law of intensity. The E.M.F. obtained with the same system on extension of the experiments for longer periods. Influence of the composition of light upon the E.M.F. obtained (coloured screens). Influence of concentration of the solution upon the E.M.F. obtained.

10. The physico-mathematical theory of constant cells reversible in respect of the cation. The deduction of the general equation from the maximum work done by the system under the action of light.* An experimental test and verification of the same in all its details. The E.M.F. and intensity of light. The analogy between the action of heat and of light upon the systems, following from the direction of the current when the same plate is heated or illuminated (at a constant temperature); the author's "principle of movable equilibrium for light."

(11) Chemical statics and dynamics of constant cells reversible in respect of the anion (*e.g.*, Ag-BrAg plates in NaBr solution). The composition of such a system. The reactions going on in such a system. The mechanism of the reactions. Proof that such a system is reversible, that its composition remains constant under the action of the current, that they are *sui generis* (points of difference from ordinary galvanic cells). The relationship to Gibb's rule of phases.

(12) Experimental results with constant cells reversible in respect of the anion, the course of the induction and deduction periods, the law of intensity, influence of composition of light (coloured screens). Acetylene and arc. Influence of concentration of the solution, influence of temperature, influence of the cation, transformation of constant cell into inconstant cells under the action of light, and *vice versâ*, the *rôle* of the electrode in light cells. Experimental determination of the electrical potentials between the illuminated and not illuminated parts of the solution; what is to be understood under "the electrode reversible in respect of the anion"; on the conditions under which constant reversible galvanic cell can be obtained, etc.

(13) The physico-mathematical theory of constant cells reversible in respect of the anion. A detailed theoretical and experimental investigation similar to that in (10).

* See 'Roy. Soc. Proc.,' vol. 74, 1905, p. 369.

(14) On the E.M.F. of constant reversible cells and the intensity of light. (Experimental proof.)

(15) Chemical velocity of reaction in homogeneous systems when they are shifted to a new point of equilibrium by light at a constant temperature, follows, after the induction period has passed, the same law of mass action as in the dark. Further extensive experimental confirmation given by "galvanic cells created by light."

(16) Chemical equilibrium in heterogeneous systems when shifted by light at a constant temperature to a new point, follow, after the induction period has passed, the same laws as in the dark.

(17) The *maximum* work (containing the constant of equilibrium) and the law of intensity. Experimental proof that in homogeneous systems

$$\mathrm{RT}\log\frac{C_1^{n_1}C_2^{n_2}\ldots}{C_3^{n_3}C_4^{n_4}\ldots} = \mathrm{RT}\log\mathrm{K} = \mathrm{RT}\log\frac{k_1}{k_2} = \mathrm{C.I},$$

where k_1, k_2, are the velocity constants, K the constant of equilibrium.

(18) The connection between the velocity of chemical reaction produced by the action of light, the intensity of light and absolute temperature: (*a*) when both reactions go on under the action of light only

$$\mathrm{RT}\log\mathrm{K}_1 = \mathrm{C''I} \text{ or } \mathrm{C''I}+(k), \quad \text{and} \quad \mathrm{RT}\log\mathrm{K}_2 = \mathrm{C'''I} \text{ or } \mathrm{C'''I}+(k).$$

(*b*) if only one reaction goes on in light, the other also in the dark,

$$\mathrm{RT}\log k_1 = \mathrm{C''I}+\mathrm{RT}\log k_2.$$

At a constant temperature

$$\log k_1 = \mathrm{C^{iv}I}+\mathrm{K_{iv}}.$$

(19) The velocity of molecular or physical reactions between different parts of the heterogeneous system produced by and going on only under the action of light, follow the law found by the author for those reactions in the dark.*

(20) Velocity of chemical reaction in heterogeneous systems produced by and going on only under the action of light follow, after the induction period has passed, the laws deduced by the author for velocity of chemical reaction in heterogeneous systems in the dark.†

* "Report British Association, Liverpool," 'Zeitsch. physik. Chemie,' 1899, vol. 30, pp. 348–368.

† 'Zeitsch. physik. Chemie,' 1899, vol. 30, pp. 371–382; 'Phil. Mag.,' 1902 (6), pp. 468–489.

*On the Overstraining of Iron by Tension and Compression.**

By JAMES MUIR, D.Sc., M.A., Arnott and Thomson Demonstrator in Physics, The University, Glasgow.

(Communicated by Professor A. Gray, F.R.S. Received November 29, 1905,— Read January 25, 1906.)

The experiments about to be described were undertaken with the view of throwing some light on the uncertainty which seems to exist as to the effect produced by tensile overstrain on the behaviour of iron when afterwards subjected to compression. As long ago as 1848, Professor James Thomson called attention to this question,† but although much experimental work has been done since then, the following quotations should serve to justify the proposed line of research. In Thurston's "Iron and Steel" (1891), it is stated that "it has been shown that the exaltation of the elastic limit in iron is not confined to the direction of the strain produced, but that it affects the metal in such a manner as to give it an exalted elastic limit with respect to subsequent strains however applied. Thus the engineer may . . . . strain his bars in tension to secure stiffness in either tension or compression, or transversely, or he may give his bars a transverse set to obtain a higher elasticity in all other directions." Ewing, in his "Strength of Materials" (1899), writes: "It may be concluded that when a piece of iron or steel (and probably the remark applies to most other metals) has been overstrained in any way—that is to say, when it has received a permanent set by the application of stress exceeding its limits of elasticity—it is hardened in the sense of being rendered less capable of plastic deformation." On the other hand, in Johnson's "Materials of Construction" (1900), the statement may be found that, "Both wrought iron and rolled steel in their normal state have 'apparent elastic limits' in tension and compression numerically about equal. If this material be stressed much beyond these limits, however, in either direction, its elastic limit in this direction is numerically raised to about the limit of the greatest stress, while the elastic limit in the opposite

* Being a note in continuation of previous papers:—

"On the Recovery of Iron from Overstrain," 'Phil. Trans.,' A, 1899.

"On the Tempering of Iron hardened by Overstrain," 'Phil. Trans.,' A, 1902.

"On Changes in Elastic Properties produced by the Sudden Cooling or 'Quenching' of Metals," 'Roy. Soc. Proc.,' August, 1902.

"On the Effects of Tensile Overstrain on the Magnetic Properties of Iron," J. Muir and A. Lang, 'Roy. Phil. Soc. Glasgow Proc.,' January, 1905.

† 'Cambridge and Dublin Mathematical Journal,' November, 1848, or article, "Elasticity," 'Encyclopædia Britannica.'

direction is greatly lower or even reduced to zero." Practically the same view is taken in Unwin's "Testing of Materials of Construction" (1899), with the addition that "the elastic limits of a material are variable limits, restricted only by this, that the range of perfect elasticity seems to be a fixed range." In an account of Bauschinger's work, given in the 'Proceedings of the Institution of Civil Engineers' (Vol. 87), the statement as to the effect of tensile overstrain in lowering the elastic limit in compression to zero is also to be found, with the curious addition that "time in these cases has little effect."

It is possible that some or all of these statements may be reconciled, but it is thought that the experiments about to be described show, at least, that further research is desirable. The experiments, although mainly performed more than three years ago, are merely of a preliminary character; but as the present writer does not see his way at present to continue the research, it is hoped that the publication of the results so far obtained may lead to the work being taken up by some other experimenter.

Before describing the compression experiments it will be necessary to consider the behaviour of iron or steel when subjected to tension tests. This probably will be done best by means of an ideal diagram, the object of the research being not so much to find what is the actual behaviour of a more or less imperfect specimen when subjected to more or less imperfect tests, as to find what is the characteristic behaviour of thoroughly good material.

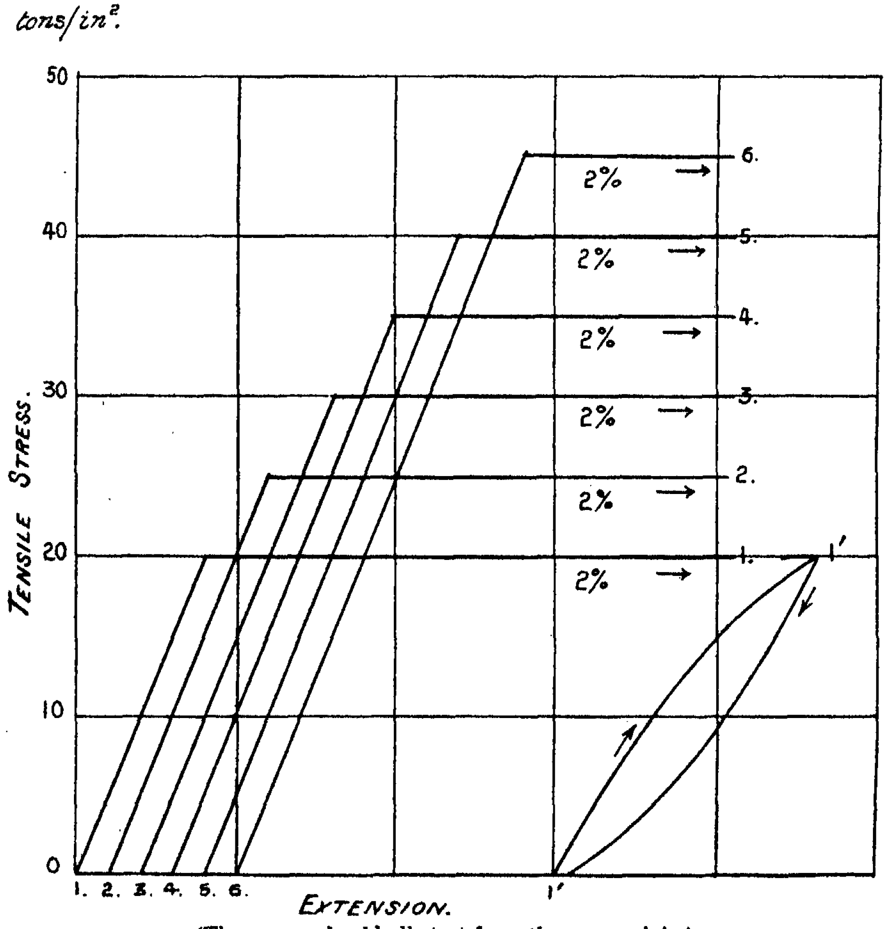
Diagram No. I, which has been modelled on experiments described in previous papers,* illustrates the elastic properties of a rod of mild steel subjected to a series of tension tests. Starting with the rod in a thoroughly annealed condition, Curve No. 1 may be taken as illustrating the primary elastic condition of the material. From the curve we see that Hooke's law is supposed to have been obeyed right up to the yield-point, which occurs at the stress of 20 tons per square inch. Many writers lay great stress on the distinction between the elastic-limit and the yield-point in iron, but as the elastic-limit was found to coincide with the yield-point in two or three actual experiments in which extensions were measured to the 1/400000 part of the length under test, it is thought unnecessary to indicate any gradual departure from Hooke's law. The yield-point indicates an important property of the material, whereas the position of the elastic-limit probably depends largely on the degree of accuracy to which measurements are made, and possibly indicates rather an imperfection of the material or of the specimen under test, than a definite property of matter.

To return to Curve 1, Diagram I, the large permanent extension which

* *E.g.*, see pp. 6, 18, 'Phil. Trans.,' A, 1902.

occurred at the yield-point is supposed to have been 2 per cent. of the length under test, and the whole extension is supposed to have occurred without alteration of the stress. It is thought that the following represents what would occur at the yield-point. At the stress of 20 tons per square inch some little portion of the material would yield. Before this little portion

DIAGRAM No. I.



(The curves should all start from the same origin.)

could again withstand the stress of 20 tons per square inch it must be allowed to stretch 2 per cent. of its length; hence there would be an instantaneous redistribution of stress across the section containing the element considered, the adjoining elements having temporarily a greater stress applied to them. If the material be homogeneous these adjoining portions must yield and the action would be transmitted piecemeal throughout the bar until the

whole had extended 2 per cent. It is evident that the total load could be reduced somewhat without causing the yielding to cease once it had started, but such reduction would not imply a lowering of the yielding stress. Hence the extension at the yield-point has been represented by a horizontal straight line. The stress-strain curve at the yield-point is usually shown of a more or less erratic form, due probably to imperfections of the material and to the rate of loading; in the present paper all curves represent static tests. The extension of 2 per cent. which is supposed to have occurred at the yield-point in Curve 1, is the least permanent extension which could be given to the bar. Of course, the load could be removed after an extension of, say, 1 per cent. had occurred, but that would mean that only one-half of the bar had yielded, the other half being in the original elastic condition.

Immediately after the bar had been extended the material would be in a semi-plastic condition. This condition is illustrated by Curve No. 1′, Diagram I, which shows no elastic-limit or yield-point, but a gradually increasing departure from Hooke's law from the lowest loads.

If the bar be allowed to rest, a slow restoration of elasticity occurs, or if the specimen be heated to say 100° C., a complete recovery from the temporary effect of overstrain may be effected in a few minutes. This treatment is not to be confounded with the process of annealing, referred to later, which requires a much higher temperature and, roughly speaking, restores the material to its original condition. Curve No. 2, Diagram I, illustrates the elastic condition of the material after recovery from the overstrain produced by just passing the primary yield-point. The yield-point is shown to have been raised by 5 tons per square inch, and the extension which occurred at this raised yield-point is 2 per cent. as before.* The material after this second overstrain would be once more in the semi-plastic state, but if restoration of elasticity be again effected by warming, the yield-point would be raised by another "step" of 5 tons per square inch, the extension at the yield-point being again 2 per cent. This process of overstrain and recovery from overstrain is supposed to have been repeated five times (Curves 1 to 6, Diagram I), the bar finally fracturing at the stress of 45 tons per square inch, the total extension being 12 per cent., neglecting the local extension at the fracture and supposing the last extension which occurred at a yield-point to have spread throughout the length under test before the neck formed at which ultimately fracture occurred. Had the bar under consideration been

* This "step" by which the yield-point is raised and the extension which occurs at a yield-point vary largely with the quality of iron or steel employed. Steps of from below 2 to 11 tons per square inch and extensions of from under 1 to 4 per cent. have been observed by the author.

broken in the usual fashion, that is, by continuous loading without allowing recovery from overstrain to take place, the breaking stress might have been, say, 35 tons per square inch, the ultimate extension perhaps 20 per cent., still neglecting local extension.

Taking, then, Diagram I to represent the behaviour of a bar of steel when subjected to successive tension tests, the question arises as to how the compression yield-point varies in correspondence with the step-by-step rise in the tension yield-point. It may be granted* that immediately after tensile overstrain "the elastic-limit in compression is reduced to zero"; the elastic-limit in tension (not the yield-point) is similarly reduced. But, after recovery from overstrain, is the compression yield-point raised or lowered? If the rise in the tension yield-point be supposed to be due to some sort of internal stress set up by the process of recovery from overstrain, then it might naturally be expected that the compression yield-point should be lowered by a step equal to the rise in the tension yield-point. But the range from zero stress to the yield-point in compression cannot be so lowered step-by-step as the tension yield-point is raised (thus maintaining a "fixed range of elasticity"), or ultimately there would result a material having a very large range of elasticity in tension, but yielding under less than no load in compression, seeing that the yield-point in tension may be raised to more than double its original amount.

The Material Employed.

The compression experiments were all carried out with specimens from a single bar of steel about 10½ feet long and 1½ inch square section. The square section was chosen, as it was intended to test by means of little cubes the elastic properties in the transverse as well as in the longitudinal direction. The steel was supplied as very mild and thoroughly annealed, yet, although one portion of the bar yielded in tension at 17 tons per square inch, another portion received no permanent set even under the high load of 27 tons per square inch. This condition of affairs might well have led to disastrous results had the bar been employed for structural purposes. The hardest portion of the bar was exactly where the maker's name was stamped, which leads one to wonder whether the bar could actually have been reheated to have the name affixed and then suddenly cooled or "quenched." Possibly the bar was simply chilled by the application of a cold die. A microscopic examination of the bar would have been interesting, but a suitable miscroscope not being to hand, the greater portion of the bar was sent

* Or see the experiment described on p. 41, 'Phil. Trans.,' A, 1899.

to a large engineering works to be thoroughly annealed in their annealing furnace. Here, again, there was disappointment. A portion of the bar which had yielded at 20 tons per square inch, and had been overstrained by a pull of 27 tons per square inch, was found, after having been returned as thoroughly annealed, to withstand a stress of 27 tons per square inch without yielding. Now, annealing after tensile overstrain ought to lower the yield-point to at least* its original value, and, by heating the portion in an ordinary chemical combustion furnace, and allowing it to cool slowly, a yield-point was got at 20 tons per square inch. Thus the material had not really been annealed by the process used by the engineers, who paid great attention to prolonged heating and slow cooling in ashes, but had little regard to the main consideration—temperature. The work was thus handicapped from the very start. It was not satisfactory to begin with a bar whose initial condition was so far from being uniform, as it was doubtful if such annealing as could be obtained with a small chemical furnace would ensure the attainment of homogeneous material.

The Apparatus Employed.

For the measurement of small elastic contractions a Ewing Compression Instrument was employed. The instrument was specially ordered by Professor Barr with view to research work of this kind. By its means a contraction of 1/125000 of an inch could be measured on lengths of specimen varying from $1\frac{1}{4}$ to 4 inches. The shortest length was always adopted, and a direct comparison being made with a Ewing 8-inch extensometer, practically absolute agreement was got between the two instruments. The compression instrument was found to be in every way satisfactory.

The main trouble in the experiments was found to be in the application of the compressive stress to the ends of the specimen. Power was obtained from the 100-ton testing machine of the James Watt Engineering Laboratory. What was required of the machine was that two rigid parallel planes should be pressed against the ends of the specimen and always remain parallel. Now, when no pressure was being applied by the machine, the compression plates (owing to slackness in fitting) were very far from being parallel, and, although it is probable that when in action parallelism was nearly attained, still, under small pressures, it could often be detected that the pressure was not being uniformly distributed over the ends of the specimen, although the ends had been planed as accurately as possible and tested with callipers. This trouble, in gripping the specimen between the compression plates,

* Annealing after tensile overstrain may lower the yield-point below its original value, see p. 28, 'Phil. Trans.,' A, 1902.

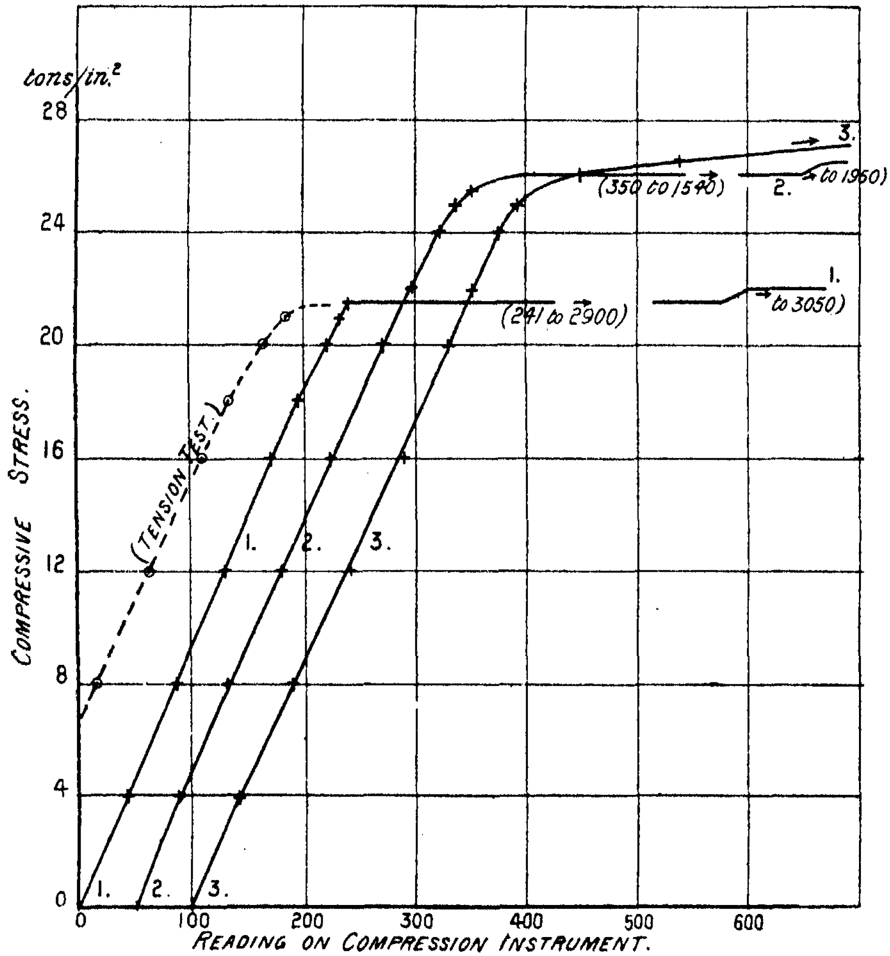
became much aggravated after a few tests had been made, as it was found that the plates became indented, in spite of the fact that the steel experimented on was very mild, and that millboard packing was inserted between the plates and the ends of the specimens. The insertion of millboard was, of course, not made with the view of protecting the machine plates, but in order to allow, if possible, free lateral expansion at the ends of the specimen under test. Several kinds of packing were tried, the best tests being obtained when the packing consisted of many sheets of paper—the leaves of an old exercise book.

The Experiments.

Diagram No. II illustrates what were, perhaps, the best compression tests obtained. The specimen employed was in the condition as supplied by the makers, and was 2½ inches long. The sides being rather over 1½ inches, the cross-sectional area was found to be 2·36 square inches. The ends of the specimen were carefully planed parallel to one another, and perpendicular to the length of the specimen. The sides were left in the condition in which they came from the maker, and so were coated with a smooth skin of blue oxide. The compression instrument was attached to the central length of 1¼ inches, the specimen was placed between the compression plates of the testing machine and load applied, first in increments of 1 and finally very slowly in increments of ¼ of a ton per square inch. Curve No. 1 of Diagram II was plotted from the readings taken, and it will be observed that Hooke's law has been accurately obeyed right up to the yield-point, which occurred at 21½ tons per square inch. Just after the load of 21½ tons per square inch was applied the compression instrument reading was 241, but shortly "creeping" was observed, and the skin of oxide began to spring off the specimen. By watching the oxide springing off, yielding was observed to spread piecemeal throughout the specimen, and finally the compression instrument showed a practically steady reading of about 2900, the load still being 21½ tons per square inch of original area. The load was increased to 22 tons per square inch to ensure that the specimen had yielded throughout, the final reading on the compression instrument being 3050. The shortening which occurred at the yield-point was thus rather over 1·7 per cent. of the length under test which indicates that the shortening which occurs at a yield-point in compression is approximately equal to the extension which occurs at a tension yield-point. A good tension test of the material in the condition as supplied was not obtained, owing to the heterogeneous nature of the bar, but the portion of the bar immediately adjoining the specimen used to obtain

Diagram II (neglecting the short length required to grip a specimen in the tension grips) was found to yield at 21½ tons per square inch, which agrees with the value of the compression yield-point.* The dotted curve in Diagram II illustrates a tension test of the material considered, and it will be

Diagram No. II.



Scale.—1 unit = a shortening of 1/125000 of an inch on 1¼ inches.

observed that Young's modulus for tension and for compression has practically the same value.

The specimen was next removed from the testing machine and placed in

* The extension observed at the tension yield-point was 0·08 or 0·09 of an inch on 4 inches, but should be less than this, as the yield-point was not well defined.

boiling water for about 10 minutes. After cooling, the specimen was remeasured, the compression instrument was re-applied, and another test made, load being again applied in tons per square inch, of actual section. Curve No. 2,* Diagram II, shows that the yield-point has been raised to about 26 tons per square inch. This yield-point was by no means so definitely marked as the last, yielding practically stopping after a shortening of about 0·8 per cent. had occurred. The load was increased to 26½ tons per square inch, giving a shortening of about 1·1 per cent., but on examining the specimen it was found to have become distinctly barrel-shaped, the ends, owing, perhaps, to lack of freedom in the lateral direction, refusing to yield; so the load was not further increased. The specimen was removed, heated in boiling water, and then planed on the four sides to remove the central bulge. Another compression test was then made, and is illustrated by Curve 3, Diagram II. Had the specimen been overstrained throughout by the second loading, then it is thought probable that the next yield-point would not have occurred till over 30 tons per square inch had been applied. Curve No. 3 shows that considerable yielding was obtained at about 26 tons per square inch, practically at the same load as before—but a load of about 30 tons per square inch was required to produce a shortening equal to that which occurred at the primary yield-point. The load was increased to 32 tons per square inch, and on then examining the specimen it was found that the ends had expanded laterally, leaving the specimen hollow in the middle instead of barrel-shaped. This establishes the fact that the ends had not been overstrained by compression in the second test.

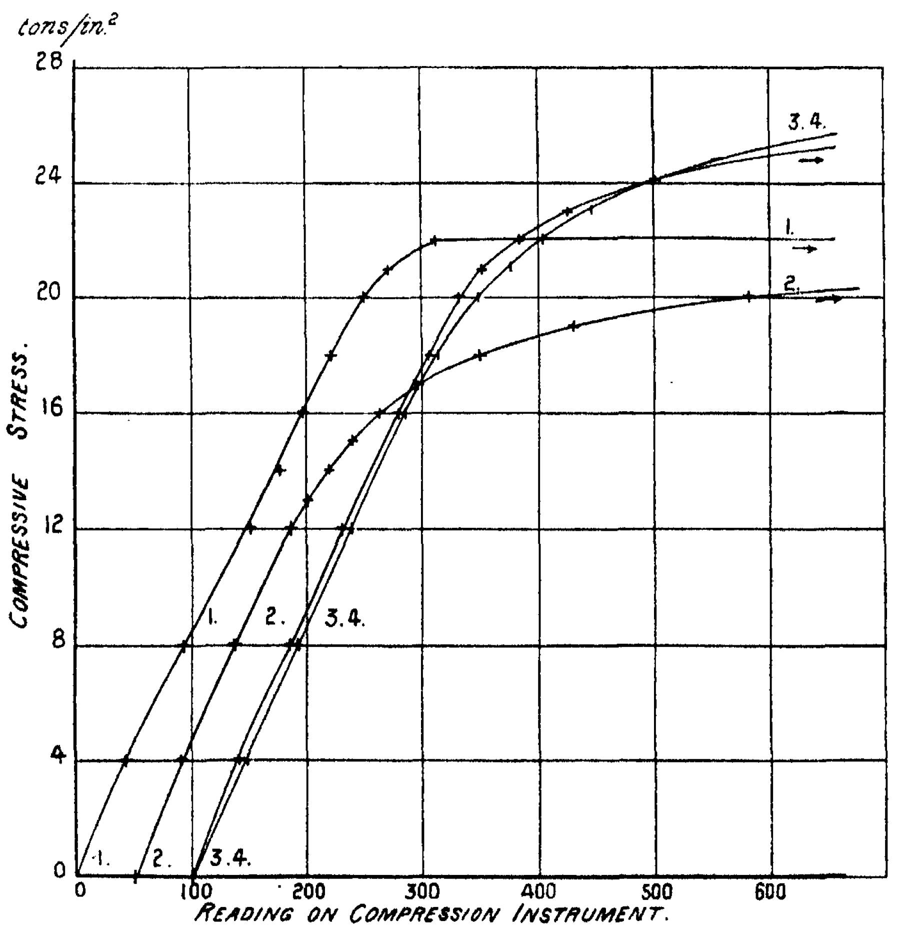
The experiments illustrated by Diagram II may be taken as agreeing with the known facts that Young's Modulus for tension and compression has the same value, and that in the normal condition of material the yield-points in tension and compression occur at practically the same intensities of stress. And further, the experiments show (perhaps not quite conclusively) that the shortening which occurs at a compression yield-point is equal to the extension which occurs at a tension yield-point, and that the yield-point in compression is raised by compressional overstrain by a "step" equal to the "step" by which the tension yield-point is raised by tensile overstrain.

The next set of experiments to be described were performed on the portion of material which had been overstrained (by a tension of 27 tons per square inch) and then annealed in a chemical combustion furnace. A compression specimen was cut from this portion of the bar, and the remainder of the

* The origin for the measurement of the shortening has been displaced, in order to avoid a confusion of the curves.

portion was then subjected to a tension test. Large yielding started at 21 tons per square inch, but 23 tons per square inch of original area, or nearly 24 tons per square inch of actual stress, had to be applied before the yielding spread throughout the bar. Recovery from overstrain was effected, and a second compression specimen was cut from the bar thus overstrained. A second tension test was then made on the remaining portion of the bar, and a well-defined yield-point obtained at 27 tons per square inch. The load was not increased beyond the yield-point, but owing to the contraction in area due to the stretch at the yield-point, the material was subjected to an actual stress of about 27½ tons per square inch. Two more compression specimens were then cut, and recovery from overstrain effected.

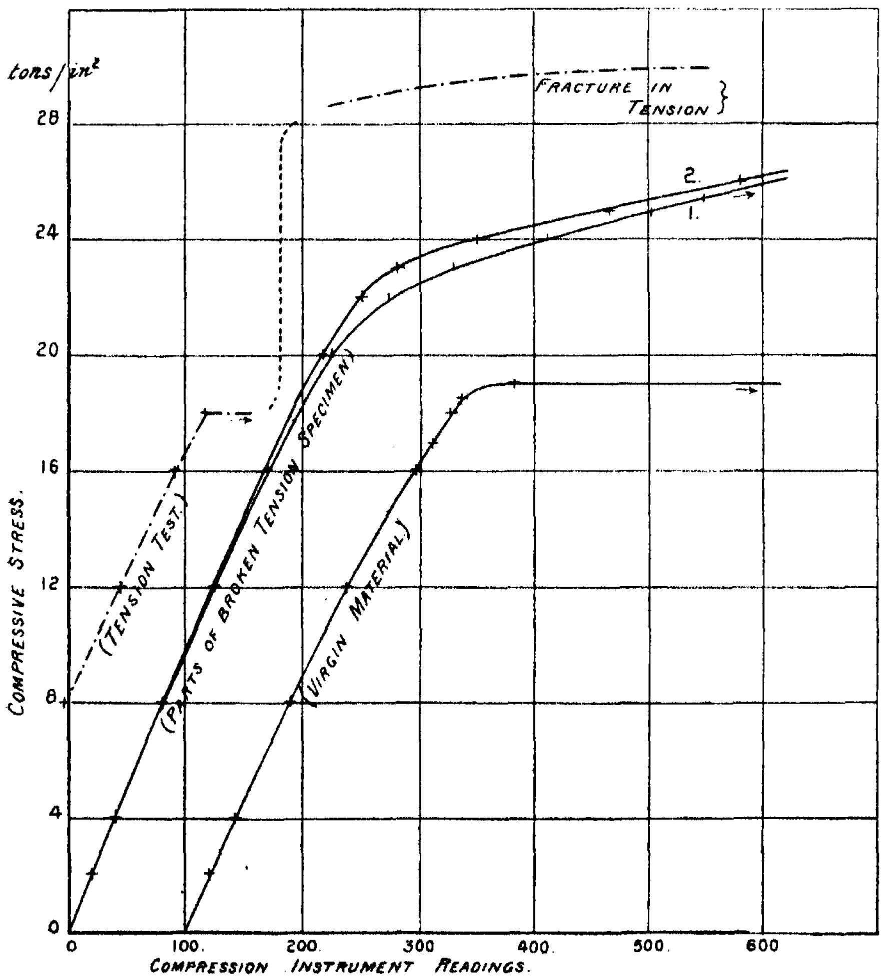
Diagram No. III.



Scale.—1 unit = a shortening of 1/125000 of an inch on 1¼ inches.

Diagram III illustrates the compression tests made on the four specimens just described. Curve 1 shows that in its original condition the material gave yield-points under tension and under compression at practically the same intensities of stress. Curve No. 2 is from an exceptionally bad test, due probably to imperfect gripping. The material was in the condition giving a tension yield-point at 27 (24+3) tons per square inch: the compression yield-point would be placed about 21 (24−3) tons per square inch. Curves 3 and 4 agree tolerably well, although they do not show very good yield-points. The position of these yield-points from an examination of

Diagram No. IV.



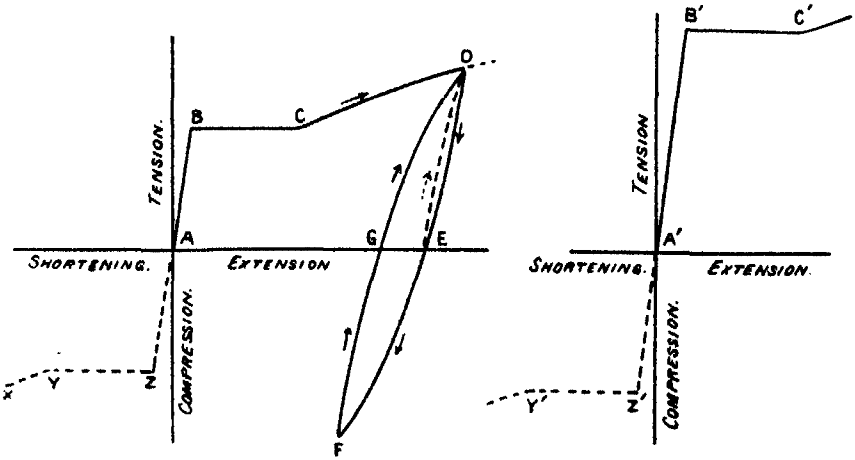
Scale.—1 unit = a shortening of 1/125000 of an inch on $1\frac{1}{4}$ inches.

the curves would be placed about $24\frac{1}{2}$ ($27\frac{1}{2}-3$) tons per square inch. The material was in the condition which would give a yield-point under tension at $30\frac{1}{2}$ ($27\frac{1}{2}+3$) tons per square inch.

The last experiment to be recorded was performed with a portion of the bar in the condition as supplied by the makers. A tension specimen was strained in the testing machine until it broke under a load of about 30 tons per square inch of actual section. The extension was about 10·3 per cent., but the break occurred in the machine grips. Two compression specimens were cut from the bar which had been thus subjected to large tensile overstrain and recovery from overstrain was effected by warming. Curves 1 and 2, Diagram IV, illustrate the compression tests made. The material was in a condition which would have given a yield-point under tension at about 34 (30 + 4) tons per square inch; the compression yield-point seems to be rather over 26 (30 − 4) tons per square inch. The large tensile overstrain seems thus to have hardened the material very considerably as regards resistance to both tension and compression, although the resistance to tension is much greater than the resistance to compression.

The experiments just described, as well as several others performed in the course of this research, give some support to the conjecture that there are two distinct causes contributing to the phenomenon of hardening by tensile overstrain. The overstraining itself—the actual stretching of the material—seems to harden the material both as regards resistance to tension and to compression, while the process of recovery from tensile overstrain, by the application of an internal stress, raises the tension yield-point above the overstraining stress, but lowers the compression yield-point below the overstraining

Diagram No. V.



stress by approximately an equal amount. A theoretical diagram (No. V) may help to make this conjecture clear. Suppose a specimen to be loaded in tension, then the stress-strain curve will be of the form A B C D. If the load be removed at D the curve may be continued, as shown by D E—the material being in the semi-plastic state. If now the load be supposed to be reversed and compression applied, the curve may be continued as shown by E F, the tensile overstrain being supposed to have hardened the material equally in both directions. The curve for compression will not be of the form indicated by A Z Y X, as the material is not in its "normal" condition. On removing the compressive stress the curve may be supposed to take the form F G, and on again reversing the stress the hysteresis cycle may be completed as shown by G D. If now recovery from overstrain be effected, elasticity is restored, the tension yield-point is raised by a definite "step" above D, while the compression yield-point is supposed to be lowered by an equal step below F. The curves A′ B′ C′ and A′ Z′ Y′ indicate the new elastic condition of the material. If this conjecture be correct then the positions of the compression yield-points may be determined, for each of the elastic conditions illustrated by Curves 1 to 6, Diagram I. In that diagram a specimen is supposed to have given yield-points in tension at 20, 25, 30, 35, 40 and 45 tons per square inch; the corresponding compression yield-points should occur at 10, 15, 20, 25, 30 and 35 tons per square inch.

Further experiments on the subject are, however, desirable, as the conjecture can scarcely be said to have been established. For example, it would be of interest to repeat the experiment of Diagram IV, endeavouring, however, to obtain a compression test with the material in the freshly overstrained condition, as well as in the condition after recovery from overstrain. By such a test it might be possible to show directly that recovery from tensile overstrain lowers the compression yield-point. Before further experiments are performed, however, new compression plates with spherical bearings and thoroughly hardened and polished faces should be designed for the testing machine.

The experimental work just described was mainly carried out by the author in the capacity of Research Student in the James Watt Engineering Laboratory.

On Mathematical Concepts of the Material World.

By A. N. WHITEHEAD, D.Sc., F.R.S., Fellow of Trinity College, Cambridge.

(Received September 22,—Read December 7, 1905.)

(Abstract.)

The object of this memoir is to initiate the mathematical investigation of various possible ways of conceiving the nature of the Material World. In so far as its results are worked out in precise mathematical detail, the memoir is concerned with the possible relations to space of the ultimate entities which (in ordinary language) constitute the "stuff" in space. An abstract logical statement of this limited problem, in the form in which it is here conceived, is as follows:—Given a set of entities which form the field of a certain polyadic (*i.e.*, many-termed) relation R. What "axioms" satisfied by R have as their consequence that the theorems of Euclidean Geometry are the expression of certain properties of the field of R? If the set of entities are themselves to be the set of points of the Euclidean Space, the problem, thus set, narrows itself down to the problem of the axioms of Euclidean Geometry. The solution of this narrower problem of the axioms of geometry is assumed (*cf.* Part II, Concept I) without proof in the form most convenient for this wider investigation.

Poincaré* has used language which might imply the belief that, with the proper definitions, Euclidean Geometry can be applied to express properties of the field of any polyadic relation whatever. His context, however, suggests that his thesis is, that in a certain sense (obvious to mathematicians) the Euclidean and certain other geometries are interchangeable, so that, if one can be applied, then each of the others can also be applied. Be that as it may, the problem here discussed is to find various formulations of axioms concerning R, from which, with appropriate definitions, the Euclidean Geometry issues as expressing properties of the field of R. In view of the existence of change in the Material World, the investigation has to be so conducted as to introduce, in its abstract form, the idea of time, and to provide for the definition of velocity and acceleration.

The general problem is here discussed purely for the sake of its logical (*i.e.*, mathematical) interest. It has an indirect bearing on Philosophy by disentangling the essentials of the idea of a Material World from the

* *Cf.* 'La Science et l'Hypothèse,' Chapter III, at the end.

accidents of one particular concept. The problem might, in the future, have a direct bearing upon Physical Science, if a concept widely different from the prevailing concept could be elaborated, which allowed of a simpler enunciation of physical laws. But in physical research so much depends upon a trained imaginative intuition, that it seems most unlikely that existing physicists would in general gain any advantage from deserting familiar habits of thought.

Part I (i) consists of general considerations upon the nature of the problem and the method of procedure. Part I (ii) contains a short explanation of the symbols used. Part II is devoted to the consideration of three concepts, which embody the ordinary prevailing ideas upon the subject and slight variants from them. The present investigation has, as a matter of fact, grown out of the *Theory of Interpoints*, which is presented in Part III (ii), and of the *Theory of Dimensions* of Part IV (i). These contain two separate answers to the question: How can a point be defined in terms of lines? The well-known definition of the Projective Point, as a bundle of lines, assumes the Descriptive Point. The problem is to define it without any such assumption. By the aid of these answers, two concepts, IV and V, differing very widely from the current concepts, have been elaborated. Concept V, in particular, appears to have great physical possibilities. Indeed, its chief difficulty is the bewildering variety of material which it yields for use in shaping explanations of physical laws. It requires, however, the discovery of some appropriate laws of motion, before it can be applied to the ordinary service of physical science.

The geometry throughout is taken to be three-dimensional and Euclidean. In Concept V the definition of parallel lines and the "Euclidean" axiom receive new forms; also the "points at infinity" are found to have an intimate connection with the theory of the order of points on any straight line. The *Theory of Dimensions* is based on a new definition of the dimensions of a space.

The main object of the memoir is the development of the *Theory of Interpoints*, of the *Theory of Dimensions*, and of *Concept V*. The other parts are explanatory and preparatory to these, though it is hoped that they will be found to have some independent value.

The Vapour Pressure in Equilibrium with Substances Holding Varying Amounts of Moisture.

Part I by Professor F. T. TROUTON, F.R.S.; Part II by Professor F. T. TROUTON, F.R.S., and Miss B. POOL.

(Received November 30, 1905,—Read January 25, 1906.)

PART I.

The knowledge of the quantity of water held under varying circumstances by substances of an absorbent character, such as cotton or woollen material, in an atmosphere of any given humidity, is not only of importance in hygrometry, but is also of general interest in connection with the processes used in drying such materials. No investigations, however, of this subject seem up to the present to have been ever published.

Some years ago, while making a series of comparative determinations of the weight of moisture absorbed out of the atmosphere by different kinds of fabrics, two interesting points were noticed. The first was that the weight of water absorbed or held by a given material under different conditions of moisture and temperature of the atmosphere, appeared to depend only on the hygrometric state (*i.e.*, the ratio of actual vapour pressure to the maximum possible), though of course the actual amount of moisture present in the atmosphere for the same ratio is very different at different temperatures. The second point noticed was that as the atmosphere varied from saturation, the temperature remaining the same, the amount of water held followed some law giving much greater reduction in weight for a given change in vapour pressure when near saturation than is subsequently obtained. As described below, this law ultimately proved to be a simple parabolic one, at least until approaching desiccation.

Early Experiments.—The first experiments were made in the following way, and though they were subsequently found not to be capable of sufficient refinement, as far as they went they served to suggest the above relations. The material used was first desiccated in the presence of air over phosphorus pentoxide, and its weight ascertained. The latter was done in the open air, so that the weight continually augmented through condensation of moisture. To get the initial weight a curve was plotted of weight against time, and extrapolated to the first moment of exposure to the atmosphere. The material was simply left exposed to ordinary atmospheric conditions, and its weight observed from time to time, the hygrometric state of the atmosphere being found simul-

taneously by a wet and dry bulb instrument. These determinations were carried on for some months, a reading being made each day. From these a few of the more striking cases are given in Table I, illustrating the first relation. These have been selected in pairs, so that the weight (W) held in the material is approximately the same, while the amount of moisture in the air is very different, as shown by the vapour pressure (p), but in each case it will be seen that the hygrometric ratio is also approximately the same, *i.e.*, the ratio of (p) the vapour pressure in the atmosphere to P the saturation pressure for the temperature (t) at the time.

This relation may then be shortly written as $W = f(p/P)$.

Table I.

W.	p.	t.	P.	p/P.
0·58	0·92	18°·4	1·14	80
0·60	0·61	7·6	0·77	80
0·53	0·82	12·6	1·08	76
0·56	0·55	6·6	0·72	77
0·52	0·46	4·8	0·64	73
0·53	0·78	12·2	1·05	75
0·42	0·85	14·8	1·24	69
0·45	0·55	7·7	0·79	70
0·36	0·59	9·7	0·89	67
0·37	0·98	17·4	1·47	67
0·30	0·97	18·2	1·55	63
0·30	0·60	10·8	0·96	63

Subsequently accurately devised experiments, carried out by Miss B. Pool, and given in Part II of this paper, have proved this relation, which indeed can be shown from thermo-dynamic considerations to necessarily hold, at least within usual atmospheric ranges.

Thermo-Dynamic Considerations.—To show this, some of the material may be imagined placed under a piston in a cylinder containing water vapour, and a cycle of the usual type passed through. Let the material holding the requisite weight of water be in equilibrium with the vapour at a pressure less than the maximum of saturation pressure. First an isothermal expansion takes place from volume v_1 to v_2, evaporation of fresh vapour from that in the material will take place, but if the amount required is small in comparison to that in the material; the pressure may be taken as remaining constant. Let w be the weight thus evaporated. The heat required for the evaporation is λw, where λ is the latent heat of vaporisation under the conditions of the operation. Secondly, let an adiabatic expansion cool the contents of the cylinder through $\delta\tau$ degrees, the pressure falling in consequence δp. Thirdly,

the volume is isothermally compressed the proper amount to admit of the cycle being completed in the fourth stage by an adiabatic compression.

We thus have, by Carnot's principle,

$$\frac{(v_2-v_1)\,\delta p}{\lambda w} = \frac{\delta\tau}{\tau}.$$

But $$w = (v_2-v_1)\,\rho p/\mathrm{P},$$

where ρ is the density of the saturated vapour at temperature τ. Hence

$$\frac{\delta p}{\delta\tau} = \frac{\lambda\rho}{\tau}\cdot\frac{p}{\mathrm{P}}; \qquad \text{but} \qquad \frac{\lambda\rho}{\tau} = \frac{\delta\mathrm{P}}{\delta\tau},$$

assuming that λ is the same in the two cases, where P is the saturation pressure at temperature T; so that we may write

$$\frac{dp}{d\tau} = \frac{p}{\mathrm{P}}\frac{d\mathrm{P}}{d\tau}.$$

That is to say, the slope of the curve* giving for different temperatures the pressure in equilibrium with the material wetted with a constant weight of water, is to that of the ordinary vapour pressure curve in the ratio of the ordinates which represent the pressures. This necessitates the pressure being proportional to the saturation pressure, or $p = \mathrm{K.P}$. That is to say, however the temperature changes, if the hygrometric state (p/P) is kept constant, the weight of water held by the material remains constant.

It is convenient to have a name for the curves on the pressure-temperature diagram, which are drawn so that the pressure is always a constant fraction of the saturation pressure. These curves throughout this paper will be called *isohygrometric* curves. It is also convenient to have a term for the curve drawn giving at different temperatures the pressure of vapour from a substance wetted with or holding a constant weight of water. These curves are called in the paper *isoneric* curves (from νηρος moisture, already in use in aneroid), or shortly *isoneres.* The considerations given above would show that approximately the isoneres coincide with the isohygrometrics. It will be shown later that in the case of solutions, when one body is non-volatile, the same relation may be considered approximately to hold.

Second Relation.—Some indication of the second relation, namely, the function W is of p/P, was got from the original set of experiments, although the range under atmospheric conditions is necessarily very limited. As typical of the general results, the following Table II is given, which is compiled from observations made with a specimen of flannel. Owing to the uncertainty of the observations, and the variation among themselves (most probably due

* See fig. 9.

to the unsatisfactory method adopted for ascertaining the vapour pressure, namely, the wet and dry bulb thermometers), they have been massed in groups round a mean value of the corresponding hygrometric states.

In the first column is given the mean hygrometric state of each group; in the second column the corresponding mean weight of the flannel; while in the third column are the calculated values of the square of the difference in weight between the latter and that of the flannel at saturation (W_1) divided by the difference between 100 and the mean hygrometric state. These are seen to be approximately constant, except in the first two or three cases. This discrepancy may be due to there having being insufficient time for equilibrium to set in, because from subsequent experiments it appears that the discrepancy from the parabolic law tends rather to the other side at low pressures:—

Table II.

H. S.	W.	$\frac{(W_1-W)^2}{100-H.S.}$	H. S.	W.	$\frac{(W_1-W)^2}{100-H.S.}$
47·4	17·334	0·038	74·6	17·588	0·052
59·1	·392	0·044	76·0	·702	0·046
62·5	·408	0·047	76·6	·616	0·054
63·5	·424	0·048	77·8	·652	0·053
65·2	·443	0·049	80·0	·692	0·056
67·3	·470	0·050	81·3	·760	0·054
68·5	·512	0·048	100·0	18·750	Saturated
70·4	·545	0·049	0	17·090	Desiccated
73·4	·572	0·052			

First Apparatus Used.—The method described being dependent upon the changes in the atmosphere, is obviously inconvenient and uncertain, as among other sources of error, during rapid changes in atmospheric conditions there is not time for equilibrium to set in. In order to make experiments under conditions of complete control in respect to the amount of moisture present in the surrounding atmosphere it is necessary to work in an enclosed space. This renders it impossible to use the wet and dry bulb hygrometer for determining the vapour pressure. A form of dew point instrument was therefore used, in which the deposition of moisture was ascertained by electrical means. This form was adopted because the conditions of the apparatus rendered it impossible to observe by eye the moment of deposition in the usual manner.

The arrangement consists essentially of two independent ribbons or wires of platinum wound at a small distance apart over the glass surface on which condensation of the moisture is to take place, and these are fused on the

glass. The outside of a test tube is used for this purpose and a circulation of iced water, maintained in the test tube by tubes leading from a reservoir, served, when required, to produce the necessary lowering in temperature for making an observation. A sensitive galvanometer and cell are connected up with the ribbons as electrodes or poles.

While dry no current is indicated, but as soon as deposition of moisture takes place one is observed. It is of interest to note that previous to the regular observational "deposition," premonitory symptoms are always to be seen in the form of slight indications given by the galvanometer.

The vessel in which the material under examination was placed was of tin plate, double walled, and had a circulation of water constantly maintained through the space between the walls from a tank or boiler kept at a constant temperature. The temperature of the tank was kept constant by means of a thermostat placed in the vessel itself, and which controlled the flame heating the boiler.

A small hole in the top of the vessel allowed of suspending the material under examination by a fine wire from a balance, placed for the purpose overhead. In this way the weight of water at any given degree of moisture of the atmosphere in the vessel could be determined. A small aperture was also provided for introducing water when it was desired to alter the hygrometric state in the vessel.

Second Apparatus Used.—From various causes no very satisfactory work was obtained with this arrangement, and it was subsequently abandoned for an apparatus in which, instead of weighing the amount of water in the material for given hygrometric conditions, the weight is gradually increased by definite increments by supplying known weights of water to the material and the corresponding equilibrium pressure observed. In these experiments, in order to avoid other sources of error, no attempt was made to keep the temperature constant by a thermostatic arrangement, as in the previous apparatus, but instead the observations were reduced to a common temperature by means of the first relation.

The method consists essentially in enclosing the dried material in a vessel freed from air, and into which can be fed as required equal weights of water until saturation is produced. The pressure of the vapour when equilibrium has been established is read after each feed, and the experimental relation is obtained, after reduction to a common temperature, giving the pressure in equilibrium with the material when holding varying amounts of water, that is to say, the isothermal on the weight-pressure diagram.

The apparatus may be described shortly as a barometer with an enlarged top to hold the wetted material under examination, and a tube with a stop-

cock serving to introduce from time to time a supply of water of measured amount to the material.

A general view of the apparatus is given in fig. 1. The bulb contains the material. From it a tube passes down into a bath of mercury to form the pressure gauge. A drying tube, A, containing phosphorus pentoxide, leads to the air pump. The measured feed of water is introduced by the tube B. The amount supplied at each feed is that held in the fine tube lying between the two taps.

It is necessary that the water so introduced should be free from dissolved air. This is clearly effected, since it is supplied from a reservoir formed by the space over the mercury at the top of a "barometer" tube, and into which air free water can be originally introduced. The method of giving a feed is to shut the upper tap, open the lower tap, and raise the mercury dish which is connected by a flexible tube. In this way the fine tube is filled with the water. The lower tap is then closed and the upper opened. The tube is connected to the bulb by a mercury sealed joint, the opening left when the tube is removed serving the purpose of introducing the specimen of material under examination.

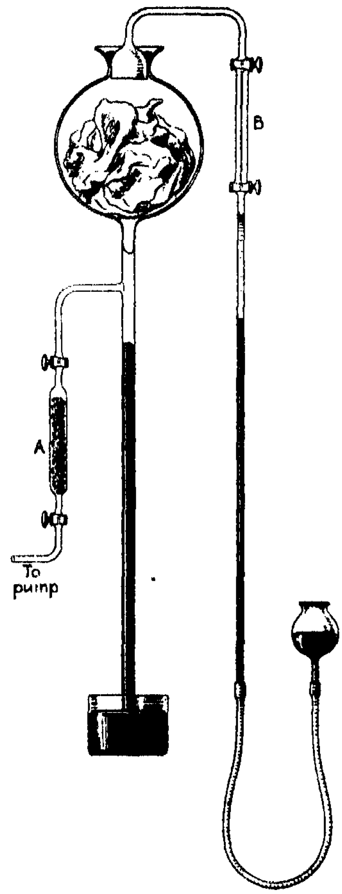


FIG. 1.

At the earlier stages of an experiment the water in the finer tube on opening the upper tap passes over, and is absorbed almost with violence, but as the material gets nearly saturated the process is slow, and it becomes necessary to drive over the water from the fine tube by carefully warming the lower end of the fine tube with a small flame.

In setting up the apparatus freshly boiled water is drawn over on top of the mercury into the feed tube, and the tap closed. On lowering the mercury dish so as to leave a space over the water, more air is invariably found to be given off. This air is then ejected through the taps. It is necessary to repeat this process a number of times before all the air is removed. In preparing for an experiment it was found to require several days to dry the material, and to get it quite free from air, which apparently continues for some time to be given off by it.

There is no further object in getting rid of all air than to avoid a troublesome temperature correction.

Experimental Results.—The results of the first series of experiments, which lasted over several weeks, are exhibited in Table III. These were made with a piece of flannel 275 square centimetres, and weighing 7·30 grammes. In the second column is given the weight of water (*i.e.*, the sum of the feeds up to the time in question) introduced into the flannel, in terms of that held by the capillary tube taken as unit. A certain amount of the water supplied was required to fill the bulb with vapour up to the pressure at the time, but

Table III.

Date.	Weight.	Vapour pressure.	Temp.	Pressure reduced to 20° C.
July 20......	0	0·109	21°·8	0·098
„ 21......	1	0·164	21·4	0·150
„ 22......	2	0·272	21·1	0·254
„ 23......	3	0·406	21·8	0·363
„ 25......	4	0·540	21·6	0·489
„ 26......	5	0·670	20·6	0·646
„ 27......	6	0·800	20·0	0·800
„ 28......	7	0·897	19·6	0·919
„ 29......	7·8	1·044	20·3	1·025
Aug. 4......	8·8	1·343	23·5	1·085
„ 5......	9·8	1·383	22·1	1·216
„ 6......	10·8	1·328	20·5	1·287
„ 10......	11·8	1·485	21·4	1·363
„ 15......	11·9	1·287	19·0	1·379
„ 17......	12·9	1·279	18·0	1·449
„ 23......	13·9	1·247	16·9	1·513
„ 30......	14·9	1·607	20·5	1·558
Sept 16......	15·9	1·315	16·7	1·617
„ 17......	16·8	1·309	16·2	1·660
„ 19......	17·6	1·286	15·6	1·669
„ 20......	18·6	1·273	15·2	1·721
„ 21......	19·6	1·223	14·5	1·730
„ 22......	20·6	1·240	14·8	1·721
„ 23......	21·6	1·227	14·1	1·736

this was negligibly small compared to the amount in each feed. In the third column is the pressure obtained after each feed. At least 12 hours were always allowed to elapse before a reading was made, so as to insure equilibrium being established. In the fourth column is the temperature, and in the fifth is given the pressure at 20° C., calculated by assuming the truth of the first relation; that is to say, by assuming that the ratio of the pressures for two different temperatures is in the ratio of the saturation pressures, provided the same weight of water is present in the flannel.

The results were found to agree satisfactorily with the parabolic law as

mentioned above. This will be seen on examining fig. 2, where the parabola $(W-w)^2 = 234(P-p)$ has been drawn, and also the experimental points from Table III. W and P are the weight and pressure at saturation. The agreement is good until the pressure falls to about 20 per cent. of the maximum value. At this point there is a distinct indication of some further action or law coming into operation. Unfortunately complete dryness had not been reached before beginning the experiments, so a second series was undertaken to further examine this peculiarity of the curve at lower pressures. The result of these experiments was to confirm the accuracy of the first

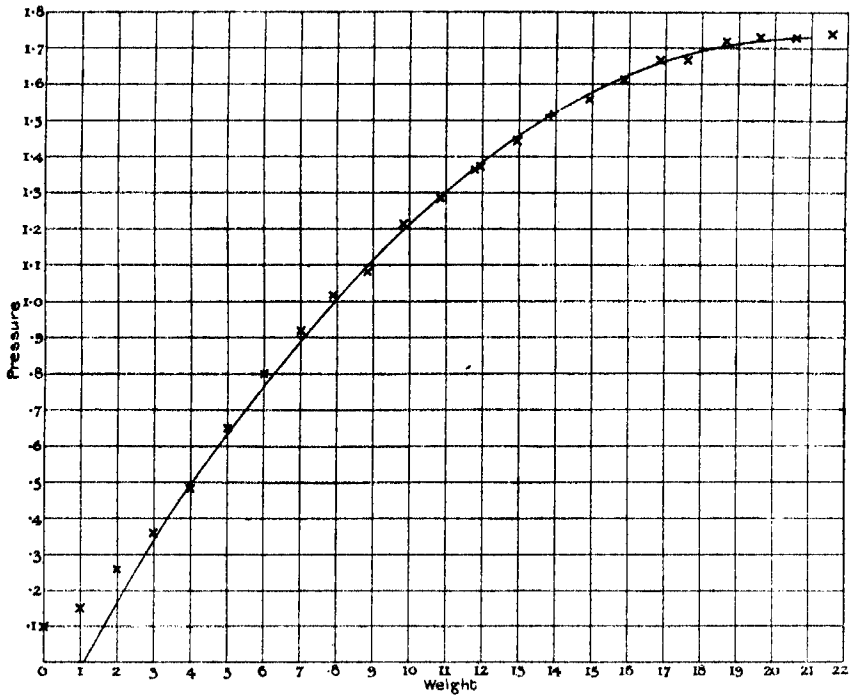


FIG. 2.—Isothermal at 20° C. for Water and Flannel.

series. The action producing this deviation from the parabolic law may perhaps be fittingly referred to as the supervening of a chemical attraction, so that at first on adding water to the desiccated material little or no vapour pressure is observed.*

Second Series.—Before the second series the drying was made complete. The observations are given in Table IV. The first three columns are the same as before. In the fifth column are given the pressures at 150°, calcu-

* [*Note added February* 17, 1906.—A similar curve has now been obtained with cotton wool.]

Table IV.

Date.	Weight.	Pressure.	Temp.	Pressure reduced to 15° C.	
Feb. 4......	0	0	12°·5	0	
„ 6......	1	0·034	14·6	0·034	
„ 6......	1	0·039	12·7	0·039	0·036
„ 6......	1	0·036	14·0	0·036	
„ 7......	2	0·074	12·7	0·075	
„ 7......	2	0·071	14·7	0·072	0·076
„ 7......	2	0·077	14·2	0·081	
„ 8......	3	0·118	12·7	0·137	
„ 8.. ...	3	0·140	14·7	0·143	0·139
„ 8......	3	0·139	15·0	0·139	
„ 10......	4	0·203	15·8	0·193	
„ 10......	4	0·206	15·7	0·197	0·195
„ 10......	4	0·200	15·4	0·195	
„ 11......	5	0·200	10·7	0·264	
„ 11.....	5	0·201	11·2	0·257	0·258
„ 11......	5	0·194	10·9	0·253	
„ 13......	6	0·278	11·0	0·360	
„ 13......	6	0·303	13·2	0·340	0·354
„ 13......	6	0·313	12·8	0·361	
„ 14......	7	0·383	13·3	0·428	
„ 14......	7	0·416	15·0	0·416	0·426
„ 14......	7	0·407	14·0	0·434	
„ 15......	8	0·491	15·5	0·476	
„ 15......	8	0·514	15·7	0·492	0·489
„ 15......	8	0·467	14·0	0·498	
„ 16......	9	0·521	13·8	0·563	
„ 16......	9	0·590	15·8	0·561	0·560
„ 16......	9	0·575	15·5	0·557	
„ 17......	10	0·678	15·4	0·661	
„ 17......	10	0·717	16·7	0·644	0·653
„ 17......	10	0·670	15·4	0·653	
„ 18......	11	0·586	11·0	0·760	
„ 18......	11	0·565	11·1	0·728	0·732
„ 18......	11	0·544	11·4	0·688	
„ 20......	12	0·592	9·6	0·843	
„ 20......	12	0·663	12·5	0·779	0·818
„ 20......	12	0·704	12·4	0·833	
„ 21......	13	0·765	13·4	0·848	
„ 21......	13	0·704	11·7	0·873	0·863
„ 21......	13	0·710	11·9	0·868	
„ 22......	14	0·644	9·9	0·899	
„ 22......	14	0·873	14·1	0·925	0·910
„ 22......	14	0·775	12·6	0·905	

Date.	Weight.	Pressure.	Temp.	Pressure reduced to 15° C.	
Feb. 23......	15	0·669	9°·7	0·946	
„ 23......	15	0·704	10·5	0·944	0·941
„ 23......	15	0·685	10·3	0·931	
„ 24......	16	0·810	12·0	0·984	
„ 24......	16	0·778	11·4	0·984	0·975
„ 24......	16	0·756	11·4	0·956	
„ 27......	17	0·736	9·4	1·062	
„ 27......	17	0·841	12·4	0·996	1·025
„ 27......	17	0·847	12·2	1·016	
„ 28......	18	0·744	9·6	1·060	
„ 28......	18	0·806	10·5	1·081	1·066
„ 28......	18	0·892	12·4	1·056	
Mar. 1......	19	0·804	9·9	1·112	
„ 1......	19	0·887	11·6	1·107	1·113
„ 1......	19	0·960	12·6	1·121	
„ 2......	20	1·002	12·6	1·170	
„ 2......	20	1·020	12·7	1·184	1·168
„ 2......	20	0·998	12·8	1·150	
„ 3......	21	1·029	12·5	1·209	
„ 3......	21	1·124	14·3	1·176	1·193
„ 3......	21	1·031	12·7	1·196	
„ 4......	21·9	0·792	8·4	1·212	
„ 4......	21·9	0·814	8·4	1·248	1·211
„ 4......	21·9	0·769	8·4	1·176	
„ 6......	22·9	0·918	10·4	1·231	
„ 6......	22·9	1·153	13·8	1·238	1·224
„ 6......	22·9	1·128	13·9	1·204	
„ 7......	23·9	0·993	12·1	1·190	
„ 7......	23·9	1·205	14·4	1·246	1·231
„ 7......	23·9	1·143	13·3	1·268	
„ 8......	24·7	0·952	10·7	1·259	
„ 8......	24·7	0·992	11·5	1·240	1·243
„ 8......	24·7	1·196	14·2	1·230	
„ 9......	25·7	1·220	14·5	1·253	
„ 9......	25·7	1·200	14·1	1·262	1·257
„ 9......	25·7	1·146	13·5	1·255	
„ 10......	26·7	1·034	12·7	1·249	
„ 10......	26·7	1·158	14·1	1·270	1·262
„ 10......	26·7	1·166	14·3	1·268	
„ 11......	27·7	1·016	12·2	1·267	1·267

lated as previously by utilising the first relation. Three observations were made in each case, but in plotting the points in fig. 3 only the mean of each three is used. This series of observations does not lie on a continuous curve so satisfactorily as the first does. The first series was made during the

months of July and August, when the temperature of the room in which the observations were made did not change rapidly, while the second series was obtained during February and March, when through artificial heating there was a greater daily range in temperature. A small error in temperature corresponds to a considerable variation in vapour pressure, and such an error is more likely to occur with rapidly falling or rising temperatures.

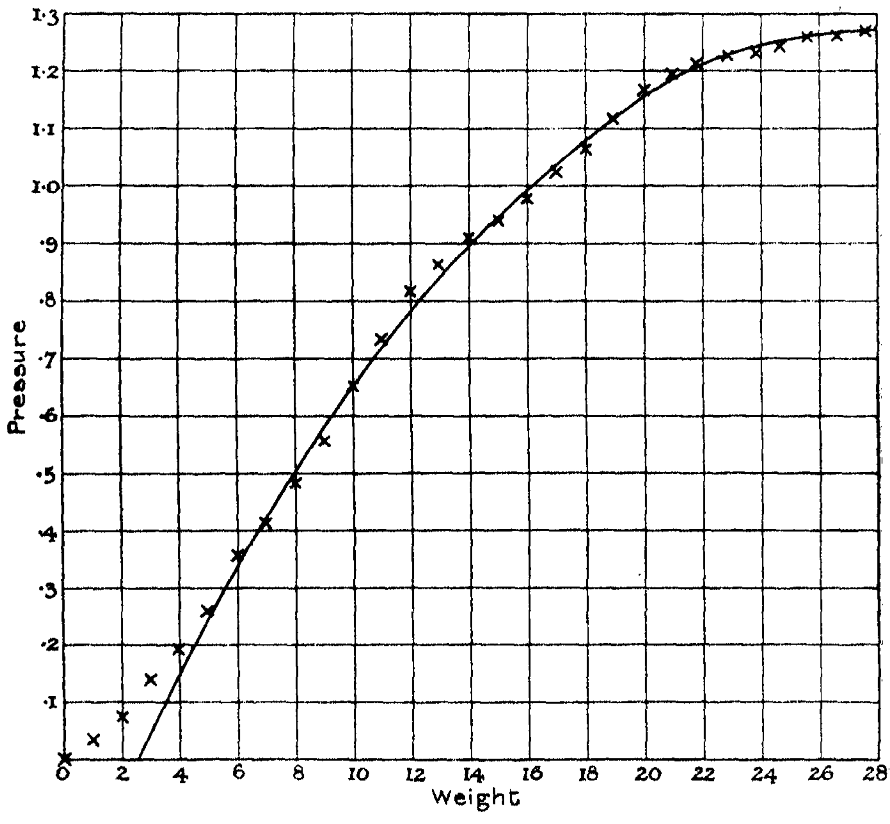


FIG. 3.—Isothermal at 15° C. for Water and Flannel.

A parabola $(W-w)^2 = 500\,(P-p)$ was found, allowing for the irregularities mentioned, to fit the points until the pressure fell to about one-fifth the maximum. This curve then, until reaching that point, may be taken as being the isothermal at 15° on the pressure-weight diagram for flannel.

On Capillary Action in the Absorbing Material.—The question is of interest as to how or in what way the water is held by the flannel or other material. No doubt some of it is held in the fine pores and cavities of the individual fibres acting as capillary tubes after the manner suggested by Lord Kelvin

many years ago, as well as in those spaces lying between and formed by two crossing fibres. The following considerations show, however, that these, all put together, can only account for a small portion of the moisture except in the case of practical saturation, and that at lower pressures the moisture must be held by simple condensation on the surfaces of the material.

We can consider the cavities in the material, especially those between interlacing fibres, as equivalent on the average to a certain conical, or rather cuspoidal cavity, which gradually fills up with water as the pressure of the vapour increases. Though we may not know the shape of this cavity, we can, following Lord Kelvin's theory, calculate the diameter of the point to which water reaches in it for each given pressure. Thus

$$R = 2T \Big/ \left\{ P_0 \frac{\rho}{\sigma_0} \log \frac{P_0}{p} - (P_0 - p) \right\},$$

where T is the surface tension at any selected temperature, t^0 C., ρ the density of water, σ_0 the maximum density of the vapour, and P_0 the maximum pressure at the temperature t^0.

If we knew the shape of the equivalent cuspoidal cavity we could arrive at the law connecting the weight held with the pressure. Assuming for example that the cavity is conical, we have for the law connecting W and p,

$$W = \tfrac{4}{3} T \pi \rho \cot \theta \Big/ \left\{ P_0 \frac{\rho}{\sigma_0} \log \frac{P_0}{p} - (P_0 - p) \right\},$$

when 2θ is the angle of the cone.

This when plotted gives us a curve which in its general trend agrees with the experimental curve, for it is tangential to the axis of W at the origin, and is asymptotic to the saturation line. Unfortunately, however, for the capillary theory, if we calculate the radius at any pressure at all removed from saturation, we get a value which is of molecular dimensions, and at which all ideas of surface tension become unsuitable. Thus for a pressure of 90 per cent. of the saturation pressure the radius at ordinary temperatures comes out to be about $1{\cdot}2 \times 10^{-10}$ cm.

Vapour Pressure from Solutions.—It is of interest to examine the pressure of the vapour of water from solutions in liquids which themselves have practically no vapour pressure, to see if similar relations hold to those we have seen to hold for wetted solids.

Taking Regnault's results for solutions of water in sulphuric acid, it will be found that similar relations do approximately hold.

In Curve 4 is plotted the isothermal at 15° C. for water and sulphuric

acid, that is to say, the vapour pressure at 15° from solutions of various strengths. The similarity of the general trend of this curve to that obtained with flannel will be noticed.

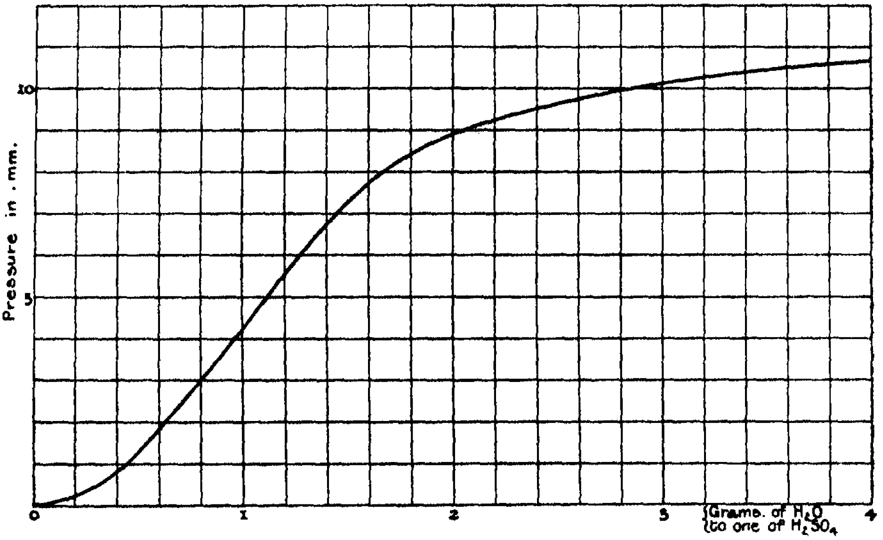


FIG. 4.—Isothermal at 15° C. for Water in Sulphuric Acid.

In the case of the solid the curve meets the saturation line at a finite distance, whereas with the liquid the approach is asymptotic. This variation in character is to be attributed no doubt to the constraint introduced by one substance being solid, so that with further accretion of water a distance is reached at which the particles of added water are outside the range of molecular attraction of the solid. On the other hand, owing to the freedom of diffusion, this constraint is not present with liquids.

The isoneres for water vapour from solutions of sulphuric acid and water are shown in fig. 5 as dotted lines. The isohygrometric curves which start from the same points at the temperature of 5° C. are also shown. These are the curves drawn with full lines.

Inspection shows that, as in the case of flannel, the two series of curves may be safely assumed to coincide, at least within the range of temperature through which Regnault worked.

Path of the Isoneres above the Critical Temperature.—At present there is no case in which we have data to enable us to draw the isoneres at relatively high temperatures. The isohygrometrics can be drawn up to the critical temperature, beyond which point they cease to have meaning. It is obvious that in general the isoneres must extend beyond this temperature. That is

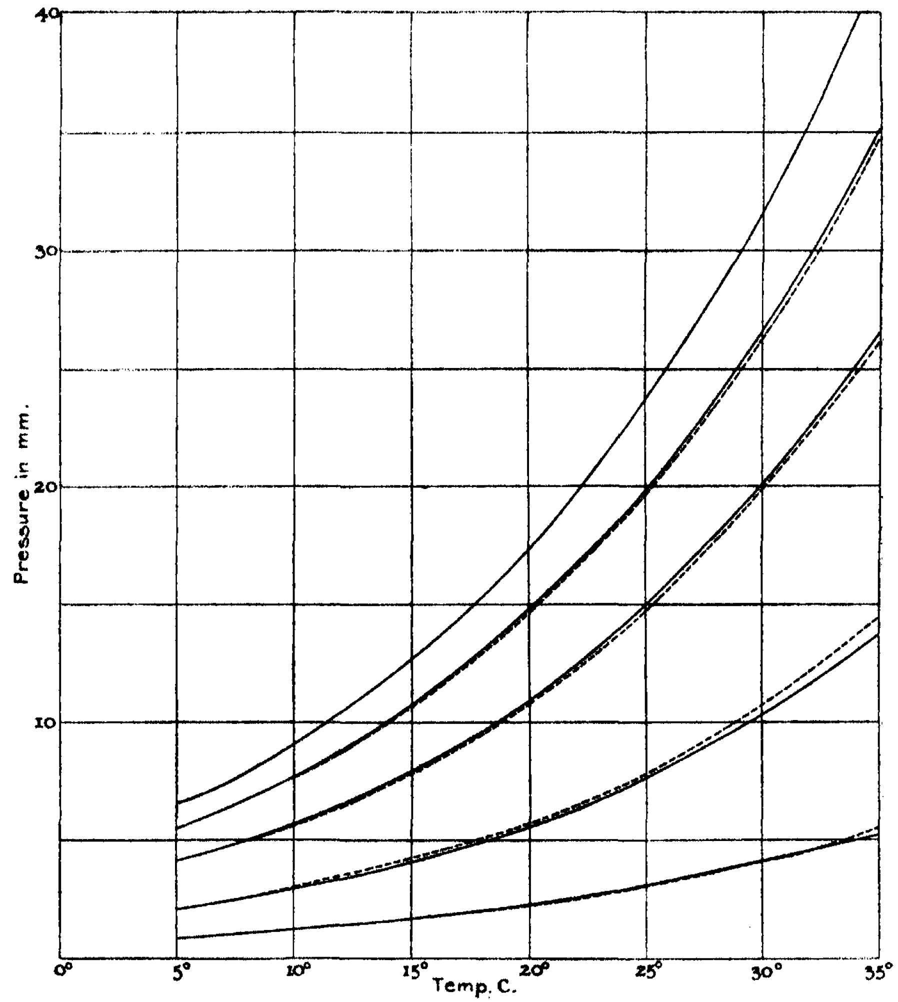


FIG. 5.—Isomers for Water and Sulphuric Acid.

to say, a curve could always be drawn giving the pressure at temperatures above the critical necessary to condense or keep from evaporating a definite weight of water on the given material. We have little information as to the condensation of water on surfaces at temperatures above the critical, but we know that similar condensation occurs with other substances, for instance, air and other gases on glass, charcoal, etc., at ordinary temperatures.

We get with some probability an idea as to how the isoneres may run at temperatures above the critical from Henry's law of the absorption of gases by liquids, namely, that the volume absorbed is constant. Thus with rising temperatures the pressure is proportional to the absolute temperature. If

this is true in the case of gases and vapours condensing on solids, the isoneres at temperatures above the critical would run in straight lines passing through the origin of co-ordinates.

The exact form the isoneres take is impossible to say without further data than are at present to hand, but we may with fair probability, as indicated above, sketch out their general character. This has been done in fig. 6 for water condensing on a solid. The top line is the vapour pressure up to the critical temperature. The curved portions of the three beneath are isohygrometrics at $\frac{3}{4}$, $\frac{1}{2}$ and $\frac{1}{4}$ the saturation pressure. Assuming the coincidence of the isoneres with these, and in addition the extension of Henry's law, we have the complete isoneres as shown.

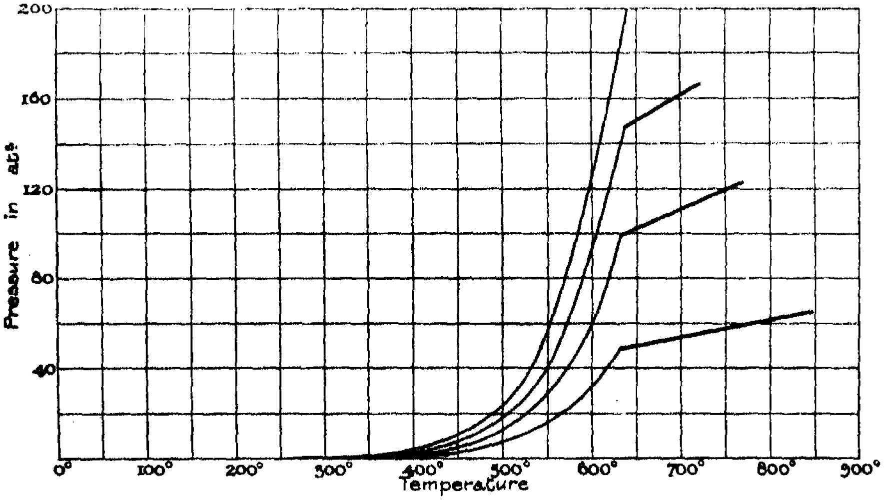


FIG. 6.—Isomers for Water Vapour.

If we follow one of these from low temperatures upwards, we see at once the enormous pressures necessary in order that a given surface may retain its layer of condensed material intact as the temperature rises.

A material such as charcoal, which condenses gases freely, will hold a certain weight of gas at any temperature and its corresponding pressure as given by its isoneres. If one follows an isonere from low to high temperatures, one can trace the character of the action of the charcoal in Sir James Dewar's beautiful method for obtaining high vacua by means of condensation in charcoal. It will be now seen how a weight of gas which would require an enormous pressure to cause it to condense on a given surface at high temperatures, can be held by the same surface at low temperatures at an exceedingly low pressure.

The Attraction of the Solid.—When the isoneres continue beyond the critical temperature, as is the case with air on glass, the attraction between the particles of the solid and those of the liquid must be supposed greater than that between the particles of the liquid itself. To take a simple case, imagine first a drop of the liquid in equilibrium with its vapour. The pressure cannot be lowered without evaporation taking place, but if we now suppose the central part of the drop replaced by, say, glass, leaving only a thin layer of liquid of a certain thickness, the pressure can be reduced, without evaporation occurring, to a definite value depending on the thickness of the layer of liquid left. Besides doing this we may pass along the isonere to temperatures beyond the critical at which the material unaided by the solid could not exist at all in the liquid state.

Some substances, for example resinous solids, may act the other way in this respect, requiring a greater pressure in order to retain a layer under the above circumstances.

Drying of Surfaces by Reduction in Pressure.—The relation as found by experiment between the vapour pressure and the weight condensed on a given surface, namely, the parabolic law, gives us for the rate of loss of moisture from a given surface with reduction in pressure

$$dw/dp = \kappa/(W-w),$$

where W is the weight held at saturation. That is to say, the rate of loss with reduction in pressure is inversely proportional to the extent drying has proceeded. Looked at from this point of view, the experimental relation seems a suitable and likely one. For as drying proceeds the average distance from the solid to the remaining particles becomes less. The effect of the attraction of the solid in retaining the layer should thus be felt in increasing ratio.

If the law were known connecting the distance apart with the attraction between particles, that is to say, the Laplacian attraction, it might be possible to deduce the relations of the vapour pressure and the thickness of the condensed layer. Conversely, now that we know this relation experimentally, it suggests a possibility of determining the form of the function assumed by Laplace.

For example, let us make the supposition that inside the boundary of the liquid vapour surface the total pressure is always to be the same, that is to say, the sum of the intrinsic pressure and of the vapour pressure for every thickness of water layer is to be a constant quantity.

Thus $P + K = P_0 + K_0$, where P_0, is the saturation vapour pressure, and K_0 the intrinsic pressure due to an unlimited depth of water under the

vapour, while P is the vapour pressure for a thickness of water layer (l), and K the intrinsic pressure partly due to this water layer and partly due to the solid beneath. We have, using the notation employed by Maxwell,*

$$K_0 = 2\pi\sigma^2\int_0^\infty \psi(z)\,dz, \quad \text{and} \quad K = 2\pi\sigma^2\int_0^l \psi(z)\,dz + 2\pi\sigma\sigma'\int_0^l \psi_1(z)\,dz,$$

where $\psi(z)$ and $\psi_1(z)$ are the functions proper for water-water and water-glass respectively, and σ, σ' the "densities" of water and glass.

The thickness of the layer of water in equilibrium with saturated vapour is probably in amount about that of the "molecular range," so that for lower pressures the solid must have an important effect. Parks,† however, gives the thickness of water layers on glass and other substances to be from 13 to 80×10^{-6} cm., which is greater than the commonly adopted value for the molecular range, though it is true that Quincke estimates the molecular range to be of about the same order, 50×10^{-6} cm. The method adopted by Parks makes it possible that some of the water supposed by him to be in the water layer was really held in capillary cavities of the character referred to earlier in this paper.

We may write

$$P_0 - P = 2\pi\sigma\left\{\sigma'\int_0^\infty \psi_1(z)\,dz - \sigma\int_0^\infty \psi(z)\,dz\right\}.$$

This gives us

$$\frac{dP}{dl} = 2\pi\sigma\,\{\sigma'\psi_1(l) - \sigma\psi(l)\}.$$

Assuming our experimental relation $(P_0 - P) = a\,(l_0 - l)^2$, where l_0 is the maximum thickness reached by the layer at saturation, and a is a constant, we have, if we take

$$\psi_1(l) = \psi(l), \qquad (\sigma' - \sigma)\,\psi(l) = b\,(l_0 - l).$$

This is the attraction per unit mass at a point situated at distance l from the plane surface of a substance of "density" $(\sigma' - \sigma)$ extending to infinity. Thus on the supposition made the law of force between two particles, as assumed by Laplace, must be such as to produce a resultant force $F = b\,(l_0 - l)$ at distance l from an infinite plane boundary of the substance, when l_0 is the molecular range or distance at which the force is insensible.

Explanation of Action of Hygroscopes.—The law of the coincidence of the isoneres and isohygrometrics throws light on the working of the various

* 'Ency. Brit.,' art. "Capillarity."

† 'Phil. Mag.,' vol. 5, p. 517.

forms of hygroscopes, in which the alteration in length of hair or gut is employed. Thus these substances are wetted to the same extent for the same hygrometric state, so that, neglecting temperature effects and supposing the length to depend simply on the wetness, we can understand how these instruments afford approximately correct indications of the hygrometric state at all temperatures.

The author has utilised directly the weight of moisture condensed on flannel for the purpose of constructing a recording hygrometer. The flannel is suspended on a recording balance. Its alterations in weight are thus registered. The amount held by the flannel alters with the temperature supposing the moisture in the atmosphere to remain constant, or supposing the temperature constant it alters with the amount of moisture, but, as we have seen, both may alter without changing the weight, provided the alteration is along an isohygrometric.

PART II.—*Measurement of the Vapour Pressure of Water at different Temperatures in Equilibrium with a Fabric wetted with a Constant Quantity of Water.*

The form of apparatus shown in fig. 7 was used by us in our experiments to investigate the relation between the temperature and the corresponding vapour pressure in equilibrium with a fabric wetted with a constant quantity of water. The material used was flannel, and the results obtained show that, at all temperatures within the limits of the experiments, the pressure of the vapour from it, when holding a constant weight of water, may be taken as being always the same fraction of the saturation pressure of water vapour.

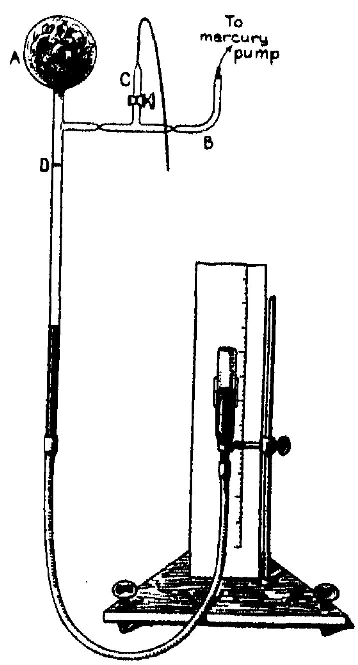


FIG. 7.

The details of the apparatus are as follows :—A is a glass bulb of about 300 c.c. capacity for containing the material under examination. B is a side tube leading to the air pump with which the apparatus was evacuated. It was sealed off at the constriction when there was a good vacuum. C is a side tube to this provided with a tap, and ending in a bent-over sealed capillary. This served for the entrance of water.

D is a fine hair tied round the stem (in a file mark) which served as a rough reference mark. The bulb is connected to a mercury manometer, the mercury level in which is read by means of a mirror at the back of the reservoir, with a fine line scratched across it. This line is made to coincide with the reflection of the mercury level and with the mercury level itself. The position on the scale is then read.

The heating arrangements (fig. 8) were as follows:—The bulb and the stem to below the mark were enclosed in a copper water bath, a rubber

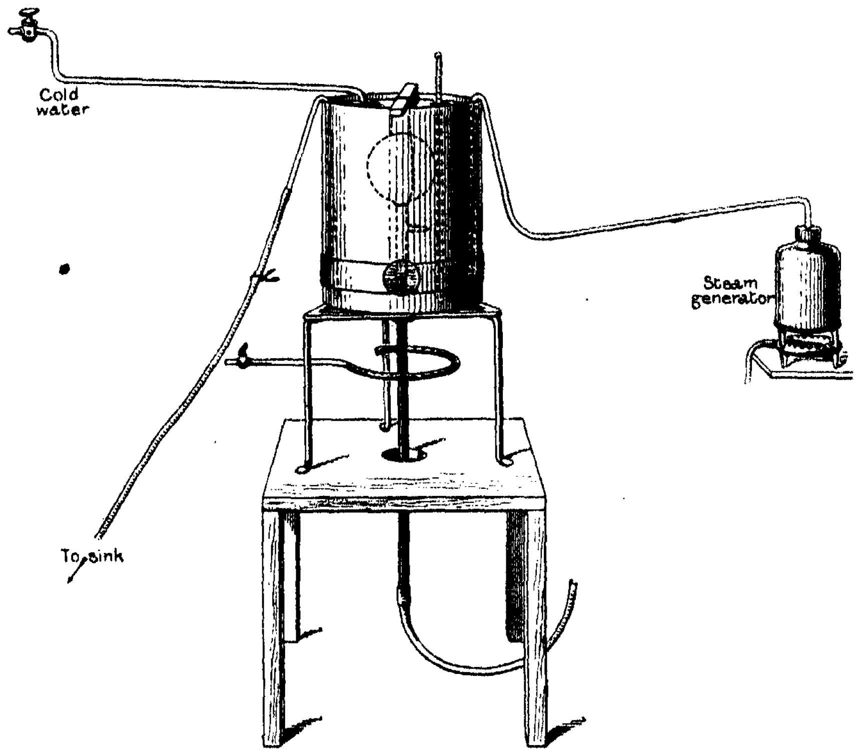


Fig. 8.

cork fitted into a short tube at the bottom allowed the rest of the barometer tube (of which the bulb was to form the top) to project below from the water bath. The mercury level during an experiment was always kept up to the mark; this part of the stem was always at the same temperature as the bulb, so that no condensation of steam took place on the mercury surface.

Two round windows of mica were made on opposite sides of the water bath; an electric glow lamp, surrounded by a piece of oiled paper, rendered

the mercury surface visible through these windows. The horizontal cross wire of a cathetometer telescope was focussed on the barometer tube at the level of the hair; the hair served as the coarse, the cathetometer as the fine adjustment. By means of the cathetometer also this level was read on the scale of the pressure gauge, so that pressures could be read by reading the position of the surface of mercury in the reservoir, since during an experiment the upper level of mercury was kept constant.

Heating to any desired temperature was effected by means of steam, which helped also to stir the water. To aid in keeping the bath at any particular temperature, a ring-shaped gas burner placed beneath was also used. Cooling could be effected by drawing off hot water by a siphon and letting in cold from a tap. The bath was kept stirred, and readings of the pressure were taken when the temperature was steady at any particular point. The material used was about 10 grammes of freshly washed flannel. This was cut into strips and inserted into the bulb.

The method of introducing the water was as follows:—The capillary from tube C (fig. 7) was sealed off and the tap turned on. The apparatus was then evacuated by the pump, which was then sealed off. After turning off the tap of the tube C the end of the capillary was broken off under the surface of some freshly boiled distilled water in a beaker. Thus the tube, as far as the tap, was filled with water, and the required amount could be let into the apparatus without letting any air in. After it had evaporated into the bulb, the side tube was taken off at the constriction near the bulb. Then the apparatus was ready for experiment, but it will only serve to give one isonere or curve with the same weight of water present. Between each set of experiments it has to be reconstructed, freshly evacuated, and a fresh amount of water let in. The evacuation takes some time, as the air has a tendency to stick in the pores of the flannel. In several experiments a correction for residual air had to be applied.

In addition, a correction for capillarity was required owing to the size of tube employed. The chief source of error undoubtedly was uncertainty as to temperature of the flannel itself. Different values for the pressure were often got according as the temperature was reached from above or below.

The following table gives the results observed when the flannel contained nearly its maximum quantity of water. In column 1 are the temperatures, and in 2 the corresponding vapour pressures. The pressures of saturated vapour from Regnault's tables are given in column 3, and the ratio of the vapour pressures from the flannel to these is given in column 4. It will be seen that these ratios are approximately constant, and considering the difficulty of maintaining the temperature steady for long enough for equili-

brium to obtain, the agreement with the suggested law of constant ratio may be looked upon as sufficiently proved.

Table I.

$\theta°$.	p.	P.	p/P.	$\theta°$.	p.	P.	p/P.
22·3	1·77	1·99	0·88	63·5	15·14	17·48	0·87
23·2	1·95	2·11	0·92	64·2	15·95	18·05	0·88
25·0	2·10	2·35	0·89	71·7	22·14	25·10	0·88
25·4	2·30	2·41	0·95	78·0	29·00	32·70	0·89
32·0	3·30	3·53	0·93	84·9	37·39	43·15	0·87
39·0	4·75	5·20	0·91	93·2	48·32	59·27	0·82
46·7	7·41	7·79	0·95	94·0	49·00	61·06	0·80
54·5	10·68	11·47	0·93				

These results are perhaps most conveniently examined when exhibited as in fig. 9. Here the top curve is that of the saturated vapour pressure. The second is the isohygrometric obtained by reducing the ordinates of the first in a constant ratio, namely, that of the mean of those in the last column in Table I. The points marked along the curve are those in column 2, and on the whole follow the curve.

In Table II are given the observations made when the flannel contains a less weight of water.

Table II.

	p.	P.	p/P.		p.	P.	p/P.
20·0	0·815	1·74	0·47	57·5	7·35	13·2	0·55
21·6	1·00	1·91	0·52	66·1	11·47	19·6	0·57
25·5	1·30	2·42	0·53	66·2	10·88	19·7	0·55
25·6	1·23	2·43	0·50	71·2	14·19	24·6	0·53
30·0	1·67	3·16	0·52	71·4	14·84	24·8	0·59
36·5	2·42	4·54	0·53	80·9	21·78	36·8	0·58
36·8	2·34	4·61	0·50	81·2	23·14	37·2	0·62
44·3	3·52	6·88	0·51	88·1	27·89	48·9	0·57
50·9	5·49	9·62	0·55	94·2	33·15	61·5	0·53
57·1	7·58	13·01	0·56	95·3	33·71	64·1	0·52

The third curve in fig. 9 is the isohygrometric drawn with the mean ratio obtained in Table II, the points as before being marked along it.

The observations in Table III were made with still less water present in the flannel.

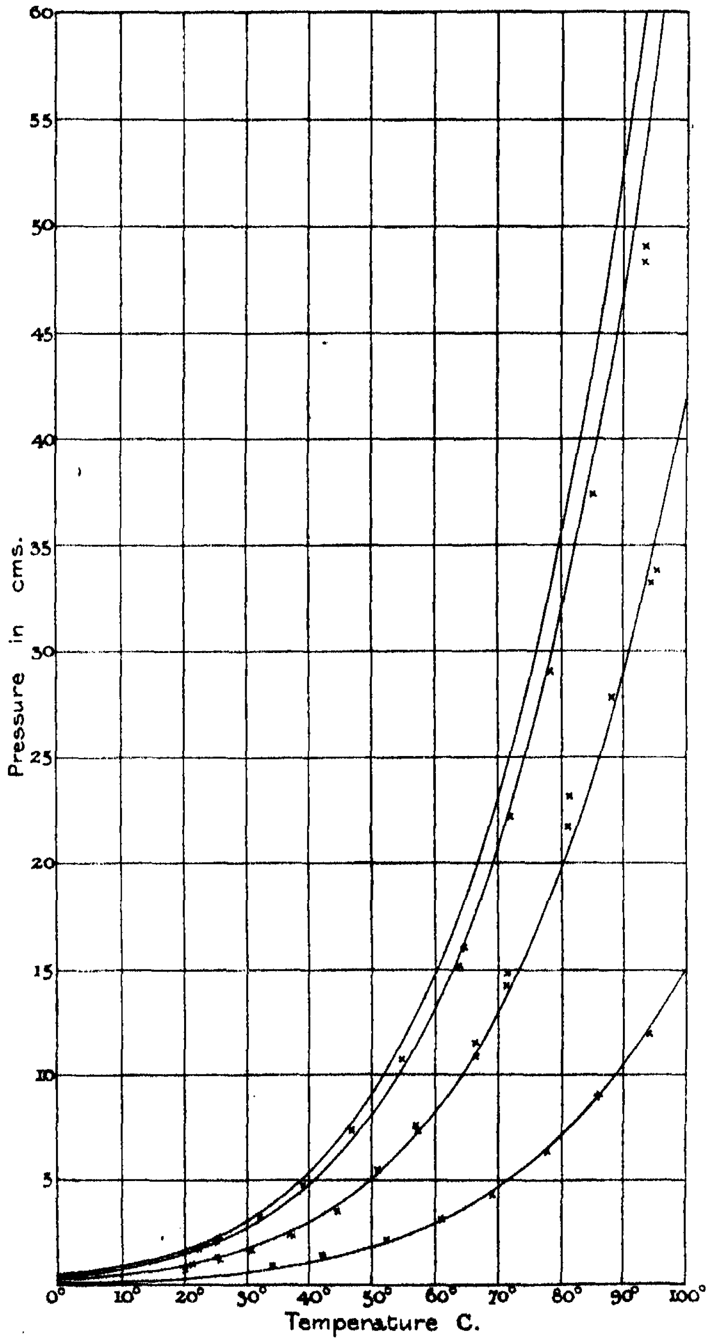

60
55
50
45
40
35
30
25
20
15
10
5
Pressure in cms.
0°
10°
20°
30°
40°
50°
60°
70°
80°
90°
100°
Temperature C.

FIG. 9.

Table III.

$\theta°$.	p.	P.	p/P.
38·9	0·97	3·93	0·24
41·8	1·46	6·04	0·24
52·2	2·15	10·25	0·20
61·0	3·16	15·59	0·20
69·0	4·31	22·34	0·19
77·6	6·36	31·17	0·20
87·8	8·96	48·30	0·18
93·7	11·94	60·39	0·19

The lowest curve is the isohygrometric plotted from the mean ratio of Table III. In this case the agreement of the points with the curve appears remarkably good, considering the experimental difficulties.

Observations and Photographs of Black and Grey Soap Films.

By HERBERT STANSFIELD, B.Sc., Research Fellow of the University of Manchester.

(Communicated by Arthur Schuster, F.R.S. Received January 3,—Read January 25, 1906.)

[PLATES 2—3.]

1. Some years ago, while working as a research student, in continuation of Reinold and Rücker's work on soap films, I made measurements on a large number of black films. The conditions of the experiments were not suitable for observing the thinner black, and I did not notice it, although I knew that it had been recorded by Newton, and that Reinold and Rücker* had not only seen the two blacks together, but had also obtained electrical observations indicating that the darker black was half the thickness of the other.

Three years later, Johonnott† published the results of optical measurements of black films, which showed that the thinner black was half the limiting thickness reached by the thicker black in the process of thinning.

On taking up the subject again rather more than a year ago, I wished to become familiar with the thinner black, and with this object examined flat

* 'Phil. Trans.,' A, vol. 184 (1893), p. 513.

† 'Phil. Mag.,' vol. 47 (1899), p. 501.

vertical films, with a low power microscope and reflected light. Using first a solution of potassium oleate in water I failed to observe any sharply defined patches of thinner black, but I found that the films often exhibited several grey tints, sharply separated from one another, and apparently intermediate in thickness between the coloured part and the black. Later, with films formed from a solution of oleate of soda in water, I had no difficulty in observing the thinner black, as it forms in circular patches whose boundaries are sharply marked. With this solution several grey tints were also observed, and it was found that the change from the thicker to the thinner black was the last of a series of similar changes that take place as the film thins.

These grey tints had not, as far as I am aware, been recognised before, although Reinold and Rücker* speak of a grey colour obscuring the boundary between the black and coloured parts of a film, when an electric current was employed to thicken the film.† These new steps in the process of thinning seemed sufficiently important to justify some trouble being taken to obtain photographs, especially as the change from the thicker to the thinner black had not, as far as I know, previously been photographed. Accordingly a special film box and camera were constructed, and the photographs illustrating this paper, showing the stages in the thinning of a sodium oleate film, were obtained in February, 1905.

Since then attention has been drawn to the existence of these new films, by a paper communicated by Johonnott‡ to the American Physical Society.

2. The arrangement of the apparatus for taking the photographs is shown in fig. 1. A is the film box, L a photographic lens of $5\frac{3}{4}$ inches focal

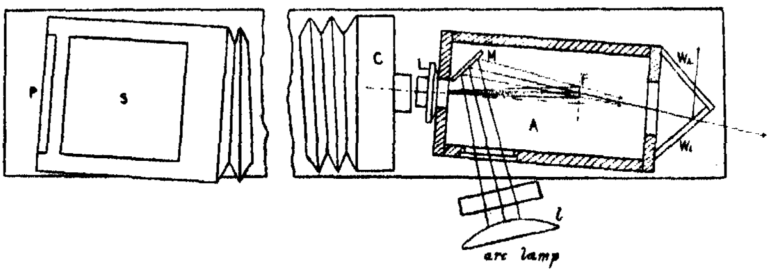


Fig. 1.

length, and C the body of the camera. Light from an arc lamp passes through a condenser and water cell, enters the film box by a plate-glass

* 'Phil. Trans.,' vol. 177 (Part II, 1886), p. 680.

† I find that the grey tints can readily be produced at the boundary by sending an electric current across it from the coloured part into the black.—15.2.06.

‡ Abstract. 'Physical Review,' vol. 20, p. 388, June, 1905.

window, falls on the mirror M and is reflected on to the film at F. The film acting as a plane mirror, reflects some of the light incident upon it into the lens L, and a magnified image of the film is formed at the other end of the camera, which is closed by the plate-holder P.

The main beam of light leaves the film box by the windows W_1 and W_2, which are arranged so that the light they reflect does not go back into the box. The plane of the film must be normal to the axis of the camera lens, if all parts are to be in focus together; the film must also be placed so that the beam of light reflected from it shall enter the lens; and as the incident light comes from one side, it is necessary, in order to fulfil both conditions, to place the film as shown in fig. 1, a little to one side of the axis of the lens.

One advantage of using oblique illumination is that the bright spot where the beam of light falls on the window W_1 is not in the field of view of the photographs.

S, fig. 1, is a horizontal focussing screen in the top of the camera box, and a hinged plate-glass mirror, silvered on the front, is usually placed so that the image is formed on the screen instead of on the photographic plate. The mirror is turned out of the way just before a photograph is taken.

The time of the exposure is regulated by a Bausch and Lomb shutter attached to the camera lens.

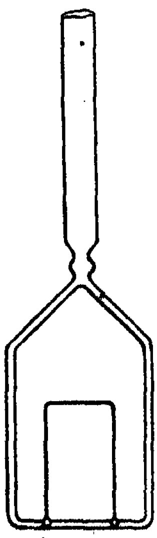


FIG. 2.

In order to prevent too rapid evaporation from the films, the film box is closed up so as to be almost airtight, and water is placed in a shallow tray covering the bottom. A water manometer is employed to test for leaks.

The films are formed on a small frame of thin glass rod, supported and protected by a stronger outer frame, as shown (actual size) in fig. 2. The inner frame is 8 mm. by 15 mm. This type of glass frame was described by Johonnott* in connection with his interferometer measurements of black films. I employed the method he describes for bending the glass rod into shape.

To make a film, the glass rod handle is pushed down until the inner frame is immersed in the soap solution in a short test tube standing on the bottom of the film box; it is then pulled up again until the frame is quite out of the solution; there is, however, always a line of liquid along the bottom of the films.

3. The photographs numbered from 1 to 4, Plate 2, show stages in the thinning of a film made from a solution of oleate of soda in

* 'Phil. Mag.,' vol. 47, p. 501.

water, 1/60. The film thinned rapidly, and only lasted five minutes, breaking soon after the change from the thicker to the thinner black had taken place. The exposure in each case was a fifth of a second.

In No. 1, the dark area in the middle of the film at the top is the thinnest of the greys; the next thicker one can be seen at each side; and, in the original, it is possible to distinguish a third, still thicker, stretching across the film below the other two. The lower edge of the thinnest grey area is pulled up in two places by fine filaments stretching across to some thicker material in contact with the glass frame. The horizontal dark bands in the photograph, lower down the film, indicate the positions of the red bands of the 1st, 2nd, and 3rd orders; the first dark band is much darker than the other two.

Close to the sides of all the photographs in this plate there are indications of the upward flow of parts of the film that have been thinned in consequence of their proximity to the glass frame.

No. 2 was taken as soon as possible after the appearance of black spots in the thinnest grey; small white specks have been formed on the advancing edges of the black areas, and they have been carried, by their weight, towards the lowest parts of the edges.

No. 3 shows the stage when the black areas have all joined together, and formed a band right across the film. The three grey films can still be traced, like a flight of steps leading down to the black; the last two steps are much clearer than the first.* Some of the white specks, or discs, have become so heavy that they have dragged down the edges on which they were formed, and have come into contact with lower edges; there they have taken up further material, until they have become circular lens-shaped thickenings, heavy enough to break away from the edges. They are seen falling down the film, leaving streaks of reduced thickness behind them.

No. 4 was taken soon after the thicker black, seen in No. 3, began to change into the thinner black. The area of thinner black spread rapidly, and a heavy crop of white discs formed on the edge.

No. 5, Plate 3, was taken a few minutes before the preceding photographs, on another film; it was accidentally given an exposure of one or two seconds, instead of a fifth of a second, so the white discs on the boundary

* *Note added February* 15.—The existence of narrow bands of grey, as shown in this photograph, would explain the bending of the interference fringes near the edge of the black sometimes observed by Reinold and Rücker ('Phil. Trans.,' II, 1883, p. 656), when they were making optical measurements of the thickness of black films. They found it necessary to assume that the black films increased in thickness near the boundary, and they calculated the thickness required to produce the displacement of the fringes observed.

between the two blacks have moved some distance, and increased in size during the exposure. It may also be noticed that the upper edge has moved further than the lower edge, this is probably due to the whole patch of thinner black rising though the surrounding thicker black. The unusually long exposure has helped to show clearly the variations in thickness of the thicker black. There are two filaments stretching down from the top of the film, across the band of thicker black, to two projections on the upper edge of the coloured part of the film. They are too fine to be visible in this plate, but their directions can be traced in the original negative.

No. 6 is a photograph of a sodium oleate film in an advanced stage of thinning, showing the grey pattern that is often formed. Almost all the coloured part of the film has gone, and the black has all changed to thinner black. The mottled parts in the middle of the film at the bottom, and supporting the grey pattern on the right, consist of collections of nodules that have formed during the thinning of the film.

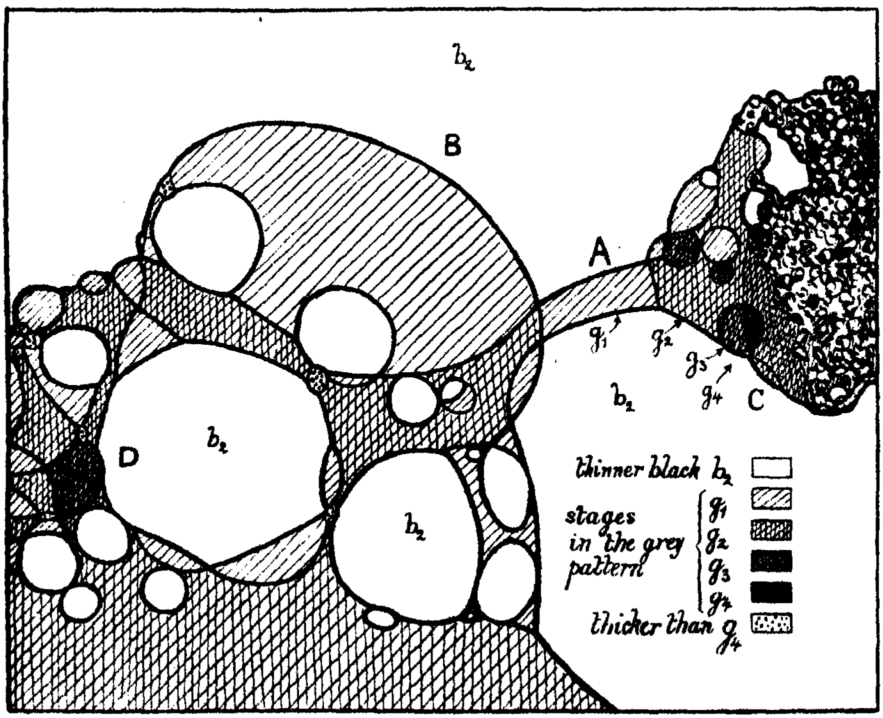


FIG. 3.

Fig. 3 is a drawing of part of the negative enlarged about three times, or rather more than 20 times the actual film, in order to show the details of the

pattern. The thinnest grey and the thinnest but one, marked g_1 and g_2, make up most of the pattern; but there are several small patches of a third grey, and part of a circular patch on the right, marked g_4, is clearly a still thicker stage.

At first sight a grey pattern often suggests that some of the thickenings are produced by the overlapping of two layers. For example, the appearance of fig. 3 suggests that the arch of first grey A stretches across the rounded area B, thickening it up to the second grey, where they overlap; and the curved edge of the patch C appears to overlap the circular patch producing the fourth grey. The impression of overlapping is, however, not supported by watching the movements of the grey pattern. If the grey films are built up of numbers of layers, the layers appear to be unable to move across one another. I have noticed nothing in the movements of the grey pattern more suggestive of stratified structure than the apparent removal of successive layers in the thinning process represented in Plate 2.

No. 7 is a photograph of a film made from a solution of potassium oleate in water, 1/60. The film was seven hours old when this photograph was taken, and, instead of thinning in the usual way, it had deposited a large amount of solid material, which is seen on the left-hand side of the film attached to the glass frame. I found that the film box was leaking badly, owing to a cemented joint along the edge of one of the windows having cracked, and this may have been the cause of the unusually large deposit of solid material.

No. 8 is a photograph of a film made from a solution of sodium oleate in water, 1/60, mixed with 40 per cent. of its volume of Price's glycerine.

It is difficult to recognise the two blacks with this solution. Large numbers of small specks are visible falling through the black; on entering the coloured part of the film they soon become considerably larger, and a number of them may be seen falling in a shower a little to the left of the centre of the film.

4. I made a number of experiments with films formed in a small airtight glass cell that could be immersed in a water bath with plate-glass sides, and kept at high or low temperatures, in order to find out whether the temperature affected the behaviour of the films. I particularly wished to try whether changing the temperature would cause the sodium oleate films to lose, or the potassium oleate films to gain, the property of changing abruptly from the thicker to the thinner black.

I did not find any differences in the behaviour of either the sodium or potassium oleate films over the range of temperature tested, which extended from 10° C. to 35° C.

These experiments showed that the hygrometric conditions are far more important than the general temperature. A film in the thicker black condition will change to the thinner black if it is allowed to lose water by evaporation, and will thicken up again if the conditions are altered so that the air becomes saturated with moisture, whether the temperature is high or low.

The thicker black is the stage to which the sodium oleate films thin when they are left shut up in a flask, or an airtight glass cell, containing some of the soap solution. If they are warmed by the heat from a source of light they are liable to change into the thinner black. I often bring about the change by focussing the beam of light from the arc lamp on to the film. The thinner black will sometimes thicken up again if the light is stopped, and it may be assisted by allowing the beam to fall on the solution instead of on the film.

If the film is thickened rapidly, the change takes place by the formation of large numbers of small circular discs of the thicker black, which, increasing in size, and falling down the film like a shower of snow-flakes, pile higher and higher, and join together, until all the thinner black is filled up.

5. The white discs that are formed on the retreating edge of the thicker black, when the thinner black is being formed, may represent the material that is removed from the thicker black in reducing its thickness; they must be produced from the thicker black, and they give some evidence as to the material of which it is formed. The discs formed are much smaller when the area of thinner black extends slowly, and in some cases only a faint stream of misty thicker material is seen falling away from the bottom of a patch of the thinner black, as it rises through the surrounding thicker black.

The thinner black often develops numbers of minute brown discs or grains soon after its formation; they appear brown in comparison with the bluish light reflected by the film. With oblique illumination they shine as bright specks on the dark background.

The light scattered by these grains causes the black part of a film, viewed by transmitted light, to look more like a solid membrane than a liquid film.

The circular lens-shaped thickenings falling through the film in photographs Nos. 3 and 4, Plate 2, are formed out of the material collected during the thinning process, as discs and irregular thickenings on the various retreating edges. In No. 5, Plate 3, part of the edge of the black has been so heavily weighted with lens-shaped thickenings that it has gone down

with them, drawing out a narrow creek of black; several small patches of black, that have also been taken down, are acting as floats.

In photographs Nos. 7 and 8, Plate 3, the lens-shaped thickenings are seen falling in much larger numbers than in Nos. 3 or 4, Plate 2, because their formation is being assisted by the continual supply of numbers of the small specks or grains that are formed in the black. These grains when they enter the coloured part of the film act as nuclei around which the lens-shaped thickenings form.

When solid material is growing in a film as shown in photograph No. 7, Plate 3, the brown specks or grains in the black can be seen shooting into the tips of the dendritic growth that projects into it. The solid material in contact with the thicker parts of the film appears to grow by catching the lens-shaped thickenings that come near to it, and perhaps also by more continuous absorption from the edge of the coloured film. The edges of a coloured film close to the frame generally become thinner than the neighbouring parts that are unaffected by it, as though the frame withdrew material from the film; and all the edge of the coloured part of the film in contact with the solid material growing in No. 7 is reduced in thickness to the first order white or yellow.

The lens-shaped thickenings are drawn towards the boundaries of the film, in the same way that bubbles floating on the surface of a liquid are drawn to the sides of the containing vessel. They generally shoot into the sides, or cross the boundary of the film into the line of liquid at the bottom; but sometimes they accumulate near the bottom of a film, and in the last stages of thinning form the collections of nodules seen in photograph No. 6.

I am inclined to think that the brown specks or grains that form in the black, and the lens-shaped thickenings, contain a much larger proportion of soap than the original soap solution; and that they consist of a soap jelly which becomes, perhaps, after some further loss of water by evaporation, stiff enough to build up the dendritic structures that sometimes grow in the films.

A film in the condition represented in photograph No. 7, Plate 3, appears to illustrate the explanation of the process of churning given by A. Pockels;* the separation of the soap in the soap film being analogous to the separation of the butter in the bubbles formed in the operation of churning.

The concentration of soap in the surface of a soap solution is a source of inconvenience in forming films. If a frame is cautiously immersed in a solution standing in a bottle, so as to disturb the surface as little as possible, and taken out again so as to lift a film from the surface, the film obtained will be

* 'Ann. d. Physik,' vol. 8, 4, p. 854, July, 1902.

impeded with thickenings from the first, and would be unsuitable for showing the regular formation of the greys and blacks. It is necessary first, in order to obtain a clean film free from thickenings, to break up the surface by jerking the frame out of the solution a few times. This precaution was taken in forming the film on which the photographs Nos. 1 to 4, Plate 2, were taken; No. 5, Plate 3, taken a few minutes before, shows a considerable amount of thick material in contact with the frame at the top, because this precaution was not sufficiently attended to.*

6. A sodium oleate film that is thinning fairly rapidly generally begins to form a grey pattern when it has developed a broad band of the thinner black across the top, and a number of lens-shaped thickenings have been formed on the upper edge of the coloured part of the field. It often happens that part of the edge in the middle of the film goes down with the thickenings on it, drawing out a narrow creek of black as in No. 5. The grey films are then first seen in the creek; they are carried up by rising patches of black and drawn out into bands, which arch over from one side of the mouth of the creek to the other, and often become very long and narrow before they break and allow the patch of black they enclose to escape. The bands of grey appear to be produced in some way from the thickenings on the sides of the creek.

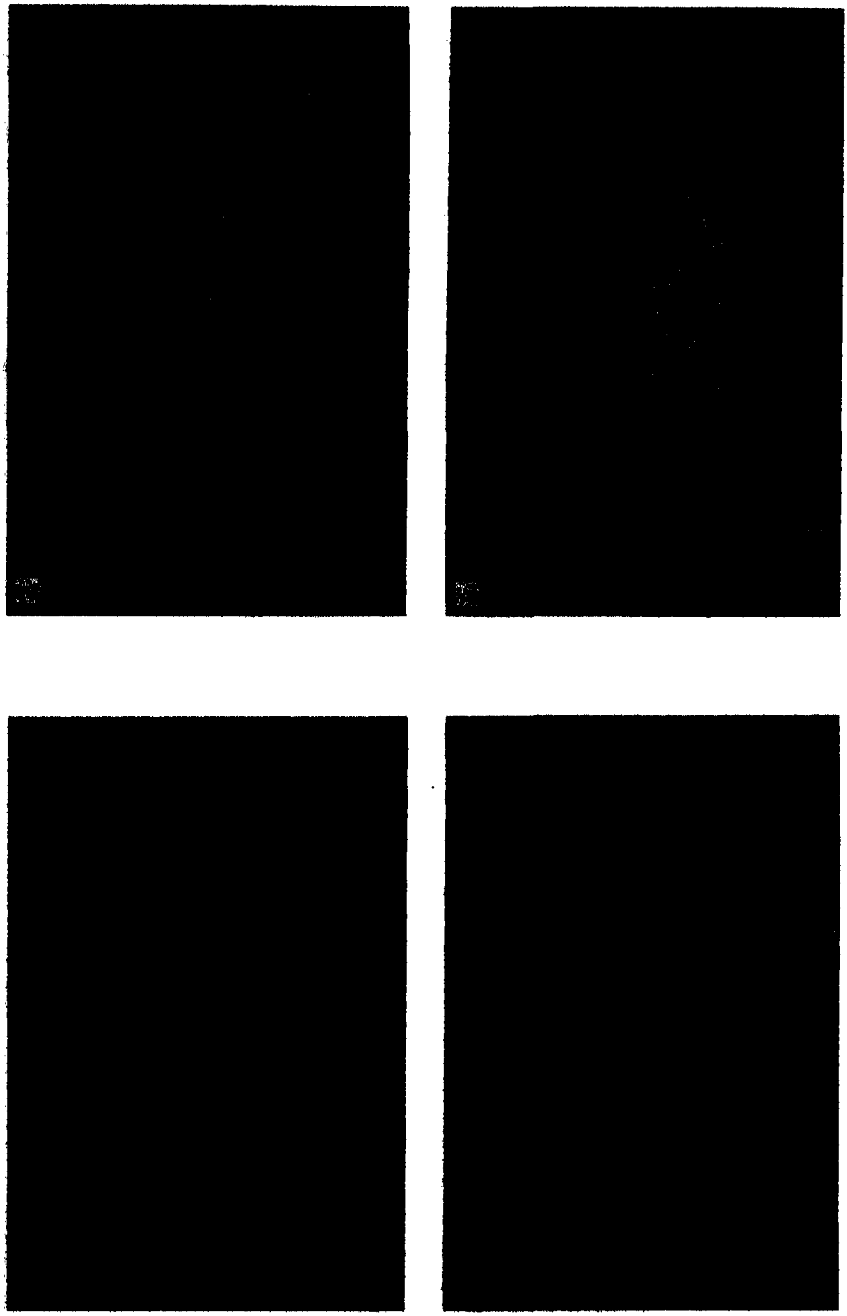
A grey pattern may also be formed under conditions such that all the black is in the thicker state. If the conditions are changed, by warming the film, so that the black thins gradually and then changes to the thinner black, the stages of the grey pattern may also undergo some slight changes and a fresh pattern be developed under the new conditions.

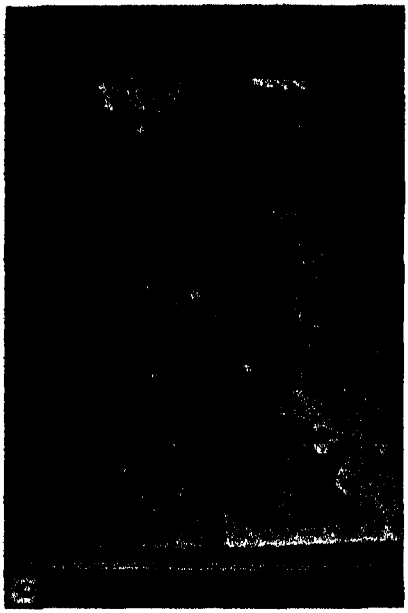
The stages that make up the grey pattern do not appear to be very closely connected with the grey stages formed during the early thinning process.

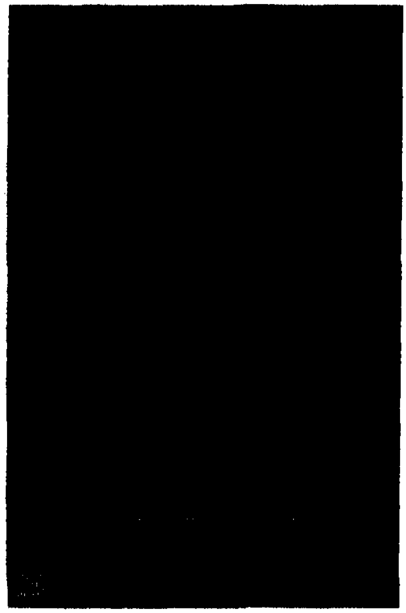
The thinnest grey in the grey pattern, formed under conditions such that all the black is being rapidly reduced to the thinner black, is not identical with the thicker black. I have seen the thinnest grey of the grey pattern in contact with areas of thicker black which were rapidly shrinking up with white specks or discs on their retreating edges. The thinnest grey was not quite so dark as the thicker black in contact with it, and did not develop white specks on its edge or appear to be affected in any way by conditions which caused the thicker black to disappear very rapidly.

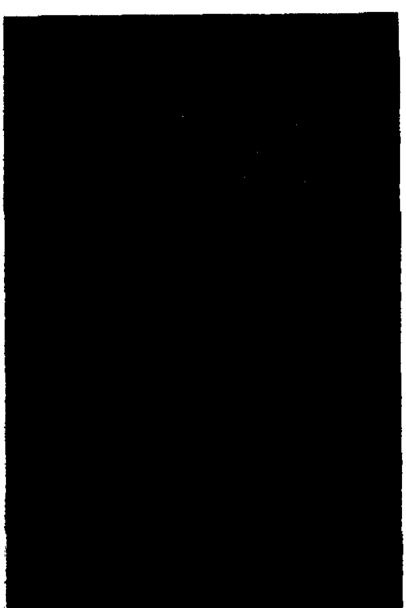
I have only observed three grey stages during the thinning process of sodium oleate films, but I have counted as many as six stages in the grey patterns of these films.

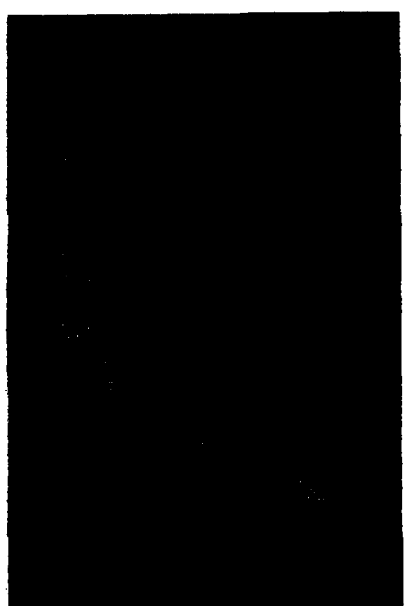
* The formation of solid pellicles on the surfaces of aqueous solutions of soaps and other substances has been described by Ramsden ('Roy. Soc. Proc.,' vol. 72, p. 156).











The stages of the grey pattern, even when they are formed under the most trying hygrometric conditions, do not show the tendency to become thinner, by breaking down into the next thinner stage, that is characteristic of the greys formed during the thinning process.

I think the evidence points to the conclusion that the grey pattern does not part with water so readily as the thicker black or the grey films formed during the thinning process.

Perhaps the explanation suggested for the lens-shaped thickenings may also be applied to the grey pattern which appears to be derived from them. The grey pattern may be formed of material that is richer in soap than the material forming the thicker black or the three grey films formed in the thinning process and may on that account not be so readily reduced in thickness by evaporation.

7. In distinguishing the three grey films formed in the thinning process, or the stages in the grey pattern, by numbers, I have called the thinnest stage number one, as the thinner stages form in relatively large areas, and are easily recognised. The plan of calling the thicker films by the higher numbers, corresponds with the numbering of Newton's orders of colours, and with the names β_1 and β_2 employed by Reinold and Rücker for the thinner and thicker black; it is, however, opposed to the method adopted by Johonnott, who speaks of the first and second black according to the order in which they are formed in the process of thinning. I find the names first and second black as used by Johonnott convenient, but have used the terms thicker and thinner black in this paper, in order to avoid any uncertainty.

In conclusion I wish to express my thanks to Professor Schuster for placing the resources of the physical laboratory at my disposal; and to Mr. F. H. Gravely for help in taking some of the photographs.

On a Property which holds good for all Groupings of a Normal Distribution of Frequency for Two Variables, with Applications to the Study of Contingency-Tables for the Inheritance of Unmeasured Qualities.

By G. UDNY YULE, Newmarch Lecturer in Statistics, University College, London.

(Communicated by Professor O. Henrici, F.R.S. Received November 4,—Read December 7, 1905.)

I. *General Theory and Illustrations from Correlation Tables.*

1. Suppose a contingency-table to have been formed for two characters A and B which have been assigned in some way into classes $A_1 \ldots A_r$, $B_1 \ldots B_s$, the table having r columns, s rows, and giving the frequencies of occurrence of all combinations like A_mB_n. Let (A_m) (B_n) denote the frequencies of A_m and B_n, (A_mB_n) the frequency of their combination.

Extract from the general contingency-table any four adjacent frequencies, say—

$$\begin{array}{ll} (A_mB_n), & (A_{m+1}B_n) \\ (A_mB_{n+1}), & (A_{m+1}B_{n+1}). \end{array}$$

Consider these as the frequencies of a table exhibiting the association between A_m and B_n in a "universe" or field of observation limited to A_m A_{m+1}, B_n B_{n+1} alone. If the ratio

$$(A_mB_n)\,(A_{m+1}B_{n+1})/(A_mB_{n+1})\,(A_{m+1}B_n)$$

be unity, A_m and B_n are independent; if greater than unity, positively associated; if less than unity negatively associated.

The whole of the contingency-table can be analysed into a series of elementary tetrads such as the above, each one overlapping its neighbours, so that an r-s-fold table contains $(r-1)(s-1)$ tetrads. Suppose the sign of the association in each tetrad to be noted. These signs may (1) vary significantly in different parts of the table, or (2) be the same throughout.

A distribution of the last kind possesses several interesting and useful properties. To adopt a word from the writers on elasticity, it may be termed an *isotropic* distribution.

2. In an isotropic distribution the sign of the association is the same not only for every elementary tetrad of adjacent frequencies, but for every set of four frequencies in the compartments common to two rows and two columns, *e.g.* $(A_m\,B_n)$, $(A_{m+p}\,B_n)$, $(A_m\,B_{n+q})$, $(A_{m+p}\,B_{n+q})$.

Suppose that the sign of association in the elementary tetrads is positive, so that—

$$(A_m B_n)(A_{m+1} B_{n+1}) > (A_{m+1} B_n)(A_m B_{n+1}), \tag{1}$$

and similarly,

$$(A_{m+1} B_n)(A_{m+2} B_{n+1}) > (A_{m+2} B_n)(A_{m+1} B_{n+1}). \tag{2}$$

Then, multiplying up and cancelling, we have

$$(A_m B_n)(A_{m+2} B_{n+1}) > (A_{m+2} B_n)(A_m B_{n+1}). \tag{3}$$

That is to say, the association is still positive though the two A-arrays are not adjacent.

3. An isotropic table remains isotropic in whatever way it may be condensed by grouping together adjacent rows or columns.

Thus, from (1) and (3) we have, adding,

$$(A_m B_n)\,[(A_{m+1} B_{n+1}) + (A_{m+2} B_{n+1})] > (A_m B_{n+1})\,[(A_{m+1} B_n) + (A_{m+2} B_n)]. \tag{4}$$

That is to say, the sign of the elementary association is unaffected by throwing the $(m+1)$th and $(m+2)$th A-arrays into one.

4. As the extreme case of the preceding theorem, we may suppose both rows and columns grouped and regrouped until only a 2×2-fold table is left; we then have the theorem :—

If an isotropic distribution be reduced to a fourfold distribution in any way whatever, the sign of the association in such fourfold table is the same as in the elementary tetrads of the original table.

5. Isotropy, therefore, is a quality that cannot be destroyed by any mode of grouping, or of extraction, of arrays. If the smallness of the number of observations in any array (in a practical case) render the discussion of approximate isotropy difficult, owing to the influence of "errors of sampling," we may either drop that array and treat those on either side of it as adjacent, or we may group it with one or more adjacent arrays. The latter process may of course conceal, but it cannot create, a departure from isotropy. As a matter of practice no correlation-table with ordinary fineness of grouping could be expected to exhibit strict isotropy; the elementary associations are too small and the probable errors too large. Some grouping is essential—as an extreme case grouping to 3×3-fold form. In such a reduced table there will be only four associations to inspect in order to determine the isotropy, viz., those corresponding to tetrads 1254, 2365, 5698, 4587 in the 1st, 2nd, 3rd and 4th quadrants of the table, taking the compartments as numbered in the order

1	2	3
4	5	6
7	8	9

Both grouping and extraction of arrays have been freely used in Part II of this paper for the discussion of approximate isotropy.

6. The normal frequency-distribution for two variables is an isotropic distribution, to which the preceding theorems accordingly apply.

Let the frequency for an interval of $\pm \frac{1}{2}\text{-}dx \pm \frac{1}{2}\text{-}dy$ round deviations of the variables x, y be given by $z \,.\, dx \,.\, dy$, where

$$z = z_0 \exp - \tfrac{1}{2}\left(\frac{x^2}{c_1^2} + \frac{y^2}{c_2^2} - \frac{2rxy}{c_1 c_2}\right), \tag{5}$$

$c_1 c_2$ being the standard-deviations of arrays and r the coefficient of correlation. Writing down from (5) the frequencies of pairs of deviations, x_1y_1, x_1y_2, x_2y_1, x_2y_2, we have for the cross-ratio of the frequencies (x_1y_1), etc.,

$$\frac{(x_1y_1)\,(x_2y_2)}{(x_1y_2)\,(x_2y_1)} = \exp \frac{r}{c_1c_2}(x_2-x_1)(y_2-y_1). \tag{6}$$

Assuming that x_2-x_1 is of the same sign as y_2-y_1, the index on the right is of the same sign as r. The sign of the association in the elementary tetrad is therefore of the same sign as r. The distribution is therefore isotropic. Every grouping of a normal distribution is therefore isotropic—be the class-intervals equal or unequal, large or small—and the sign of the association in a normal distribution reduced to 2×2-fold form is always the same, whatever the axes of division.*

7. In Professor Pearson's earlier method of calculating the coefficient of correlation for unmeasured qualities, it is assumed that the contingency-table represents a grouping, by arbitrary intervals, of normally distributed frequency.† But the assumption of normality can only be valid for some one order of the classes—at most—and the question arises whether the right order has been assigned; Professor Pearson's later method of the "contingency-coefficient" avoids all difficulty as to class-order, and this is rightly claimed as one of its chief advantages.‡

If, however, the contingency-tables dealt with were really disarrangements of normally distributed frequency, no great difficulty as to class-orders should arise. If the rows and columns of an isotropic distribution be disarranged in any way whatever, they can be rearranged in their right order, by inspection almost, owing to the following property:—

If, say, two columns in an isotropic contingency-table be isolated, the

* In a previous memoir (*vide* bibliography, p. 336, 15, 1900) I showed that this was empirically the case for a certain approximately normal distribution, but did not recognise it as a necessary consequence of normality.

† 'Bibl.,' 5, 1901; the method employed in numerous memoirs of later date (1, 2, 3, 4, 6, 7, 9, 10).

‡ 'Bibl.,' 11, 1904.

ratios of the frequency in the one column to the sum of the frequencies in the two columns, for successive rows, must form a continuous series, ascending or descending.

8. Thus, suppose we isolate the mth and $(m+p)$th A-arrays with frequencies

$$\begin{array}{ll} (A_m B_1), & (A_{m+p} B_1), \\ (A_m B_2), & (A_{m+p} B_2), \\ \vdots & \vdots \\ (A_m B_s), & (A_{m+p} B_s), \end{array}$$

we have from the first tetrad, at the head of the column, supposing the fundamental association positive,

$$\frac{(A_m B_1)}{(A_m B_1)+(A_{m+p} B_1)} > \frac{(A_m B_2)}{(A_m B_2)+(A_{m+p} B_2)}.$$

From the second tetrad

$$\frac{(A_m B_2)}{(A_m B_2)+(A_{m+p} B_2)} > \frac{(A_m B_3)}{(A_m B_3)+(A_{m+p} B_3)},$$

and so on, each ratio being less than the one above it.

If the rows have been disarranged, their right order will be given at once by the order of the ratios (subject, as always in practical cases, to difficulties created by casual irregularities of the nature of errors of sampling or otherwise).

It is assumed, of course, that the orders 1, 2, 3 ... r, r ... 3, 2, 1 are indifferent; it is, *e.g.*, mere convention whether we agree to range a series of eye-colours in order from light to dark or from dark to light.

9. This possibility of assigning the right order of arrays from the intrinsic character of the distribution, without reference to extraneous considerations, seemed so extremely curious that I judged it worth while to work out an illustration in a case where the right order was known *à priori*, so as to provide a check. The figures used are given in Table I (p. 328).

The two columns with the greatest total frequencies are 4 and 6, and these are best chosen for a first comparison as being least affected by "errors of sampling." Working out the successive ratios 53·25/(53·25+86·5) 75·75/(75·75+50), etc., we have the series of ratios (1) below. This gives as the order for the B's:—(9, 10), 3, 5, 2, 4, 1, 6, 7, 8, the correct order *inter se* of 9 and 10 being uncertain. Checking this result by a similar use of columns 6 and 9 we get the series (2), which gives the same order reversed in sign. Either A_4 and A_6 or A_6 and A_9 must therefore have been disarranged. Assuming that $A_6 A_9$ are right and that the fundamental association is positive, we may take the order as 8, 7, 6, 1, 4, 2, 5, 3, (9, 10).

As regards the last pair, a comparison of columns 4 and 8 or 4 and 9 suggests the order 9, 10 whilst a comparison of 9 with 8 (this pair being disarranged) suggests 10, 9. The preference lies slightly with the former order.

Table I.—Anisotropic Contingency-Table for Two Qualities, A and B (Table II giving the Isotropic Rearrangement).

Quality A.

Quality B.	1	2	3	4	5	6	7	8	9	Total.
1	53	15·75	5	53·25	—	86·5	—	2·5	21·5	237·5
2	16·5	1	—	75·75	—	50	4·5	22·75	65·5	236
3	1·5	1	—	10·5	—	1·5	2·5	9	11·5	37·5
4	38	14·75	2·5	106	—	90	1	17·5	53·25	323
5	4	—	—	32·5	—	11·5	1·5	20·75	34·75	105
6	35·25	12	3	11·75	3	30·5	—	—	4·5	100
7	7·25	5	1	1·75	—	8·5	—	—	0·5	24
8	1·5	1	—	—	—	1	—	—	—	3·5
9	—	—	—	3	—	—	—	3·5	1·5	8
10	—	—	—	1	—	—	—	1·5	1	3·5
Total	157	50·5	11·5	295·5	3	279·5	9·5	77·5	194	1078

Ratio $(A_m B_n)/\{(A_m B_n)+(A_{m+p} B_n)\}$.

Row.	(1) $m = 4$, $m+p = 6$.	(2) $m = 6$, $m+p = 9$.
1	0·381	0·801
2	0·602	0·433
3	0·875	0·115
4	0·541	0·628
5	0·739	0·249
6	0·278	0·872
7	0·171	0·945
8	0	1
9	1	0
10	1	0

10. Next proceeding to the quality A and dealing with rows precisely as we have done with columns, we find the following ratios for the pairs of rows 1, 2 and 2, 4:—

Ratio $(A_mB_n)/\{(A_mB_n)+(A_mB_{n+q})\}$.

Column.	(1) $n=1, n+q=2$.	(2) $n=2, n+q=4$.
1	0·763	0·303
2	0·941	0·064
3	1	0
4	0·413	0·417
5	?	?
6	0·634	0·357
7	0	0·818
8	0·099	0·565
9	0·247	0·552

Here the position of 5 is indeterminate, but remembering that B_2 and B_4 are disarranged, both series concur in giving the order for the remainder: 3, 2, 1, 6, 4, 9, 8, 7. Our general knowledge of the attributes would certainly be called into play in any practically treated case in assigning the position to A_5; the condition of isotropy alone can only offer a slight suggestion that it should lie near the upper end of the series, seeing that the combination A_5B_6 is the only one occurring and that B_6 lies near the upper end. The order may then be approximately 5 (?), 3, 2, 1, 6, 4, 9, 8, 7.

11. If rows and columns be now rearranged in the orders determined we get the comparatively orderly-looking Table II, which is, as a matter of fact,

Table II.—Showing Table I rearranged in (approximately) Isotropic Order. The numbers at heads of columns and rows refer to Table I.

Quality A.

Quality B.	5	3	2	1	6	4	9	8	7	Total.
8	—	—	1	1·5	1	—	—	—	—	3·5
7	—	1	5	7·25	8·5	1·75	0·5	—	—	24
6	3	3	12	35·25	30·5	11·75	4·5	—	—	100
1	—	5	15·75	53	86·5	53·25	21·5	2·5	—	237·5
4	—	2·5	14·75	38	90	106	53·25	17·5	1	323
2	—	—	1	16·5	50	75·75	65·5	22·75	4·5	236
5	—	—	—	4	11·5	32·5	34·75	20·75	1·5	105
3	—	—	1	1·5	1·5	10·5	11·5	9	2·5	37·5
9	—	—	—	—	—	3	1·5	3·5	—	8
10	—	—	—	—	—	1	1	1·5	—	3·5
Total	3	11·5	50·5	157	279·5	295·5	194	77·5	9·5	1078

only a grouping by 2-inch intervals of a table of Professor Pearson's for inheritance of stature from father to son (8, 1903, Table XXII). The columns should be headed "Stature of Father," "57″·5—59″·5," "59″·5—61″·5," etc. (commencing at the left), the rows "Stature of Son," "59″·5—61″·5," "61″·5—63″·5," etc. (commencing at the top). Table I was a disarrangement of Table II, deliberately effected for purposes of experiment.

12. The success of the experiment suggests that while errors will very likely be made as regards the placing of infrequent classes like A_5, the method should be able to assign positions to the more frequent classes with a considerable degree of certainty, provided always that the assumption of isotropy is legitimate.

For tables based on anthropometric measurements I think this assumption will in general hold. I tested 14 of Professor Pearson's tables for stature, span, fore-arm, and head measurements by the rough process of grouping to 3×3-fold form (§ 5), using in some cases two different groupings. The result was the same in every case, the grouped tables being isotropic without exception. The class-limits were so chosen as not to make any frequencies very small, but otherwise as much variety as possible was admitted.* It is probable enough that more extensive trials on similar material will yield some anisotropic tables, but seems unlikely that significant anisotropy will prove anything but exceptional.

II. *Applications of the Preceding Principles to Published Contingency-Tables for the Inheritance of Unmeasured Characters.*

13. The first contingency-tables to which I endeavoured to apply the principles of the preceding sections were the tables for inheritance of eye-colours, based on Mr. Galton's data, which are given in Professor Pearson's Memoir (6), 1900. In these tables the colours are classified under eight heads:—(1) Light blue; (2) blue, dark blue; (3) blue green, grey; (4) dark grey, hazel; (5) light brown; (6) brown; (7) dark brown; (8) very dark brown, black. The tints, 1, 5, and 8 are relatively infrequent, at least in the tables for direct parental inheritance; as a first rough trial I therefore dropped these tints and endeavoured by the methods of §§ 8 to 11 to determine the true order *inter se* of the remaining tints 2, 3, 4, 6, 7, using

* The following are the tables cited from Pearson (8) and (9), with the groupings used as given by the limits to the central class; where two are given, two distinct groupings were tested. From (8):—XXII, stature, 65·5—69·5 and 67·5—69·5; XXIII, span, 69—71; XXIV, fore-arm, 18—19; XXXI, stature, 61—63 and 62—64; XXXII, span, 62—64; XXXIII fore-arm, 16—17. From (9):—E_1, cephalic index, 78—80 and 78—82; E_2, ditto, 78·5—81·5: F_1, head-length, 182·5—186·5 and 178·5—190·5; F_2, ditto, 172·5—182·5: G_1, head-breadth, 143·5—147·5; G_2, ditto, 134·5—146·5; H_1, head-height, 121·5—129·5; H_2, ditto, 121·5—123·5.

only the four tables I to IV for inheritance in the first degree. To afford a thorough check on the results, I worked out the ratios $(A_mB_n)/\{(A_mB_n)+(A_{m+p}B_n)\}$ not for one or two pairs of columns only, but for every adjacent pair 2 : 3, 3 : 4, 4 : 6, 6 : 7. Having done this I did not think it necessary to evaluate the similar ratios for pairs of rows.

14. The results for Professor Pearson's Table I (inheritance from father to son) were most chaotic, but I persevered with the others and give the results, as regards the order assigned to the colours, in Table III. It will be seen that the orders deduced from the different comparisons in one and the same table were utterly discordant. Whatever the mathematical form of the frequency-distribution of these tables, it was clear that it could not be normal nor even a disarrangement of normally distributed frequency.

A little further inspection suggested, moreover, a certain peculiar order in the chaos: the results obtained from comparisons of the same pair of columns in different tables were much more alike than the results obtained by comparisons of different pairs of columns on the same table. Further, in 12 cases out of 16 the first place was assigned to the eye-colour of the column whose frequencies were treated as numerators, and in 13 cases out of 16 the last place fell to the eye-colour of the denominator-column.

Table III.—Result of Testing for the Order of Eye-colours, 2, 3, 4, 6 and 7, from Tables I, II, III and IV of Karl Pearson (6), 1900.

Table.	Ratio of frequencies in columns.	Order assigned.				
I. Father-son	2 : 3	2	6	7	4	3
	3 : 4	3	2	7	6	4
	4 : 6	2	4	3	7	6
	6 : 7	6	4	3	2	7
II. Father-daughter	2 : 3	6	7	2	4	3
	3 : 4	3	2	7	6	4
	4 : 6	4	2	3	7	6
	6 : 7	6	3	4	2	7
III. Mother-son	2 : 3	2	4	3	7	6
	3 : 4	3	2	6	7	4
	4 : 6	4	2	3	7	6
	6 : 7	4	3	6	2	7
IV. Mother-daughter	2 : 3	2	3	4	7	6
	3 : 4	3	2	6	7	4
	4 : 6	2	4	3	7	6
	6 : 7	6	3	4	7	2
The four tables superposed	2 : 3	2	6	7	4	3
	3 : 4	3	2	6	7	4
	4 : 6	4	2	3	7	6
	6 : 7	6	3	4	2	7

15. When the four tables were pooled together the empirical rule thus suggested held good without exception. When the ratios of column 2 to 2+3 were taken, eye-colour 2 was placed first and 3 last. When the ratios of column 3 to 3+4 were taken, 3 was placed first and 4 last and so on (*cf.* the last division of Table III). There seemed to be only one simple explanation of these results, viz., a relative excess of frequency in the diagonal compartments 11, 22, etc., corresponding to the same eye-colour in parent and offspring, as compared with the frequencies in a normal or other isotropic distribution.

16. This is in striking contrast to the result obtained for stature and other anthropometric measurements. But, as the reader will probably remember, Mr. Galton affirms that, judging from his material,* "stature and eye-colour are not only different as qualities, but they are more contrasted in hereditary behaviour than perhaps any other common qualities. Parents of different statures usually transmit a blended heritage to their children, but parents of different eye-colours usually transmit an alternative heritage . . . . . . if one parent has a light eye-colour and the other a dark eye-colour, some of the children will, as a rule, be light and the rest dark; they will seldom be medium eye-coloured, like the children of medium eye-coloured parents." Professor Pearson, working on Mr. Galton's data, concurred, with some qualifications, in this conclusion ((6), 1900, p. 117 and p. 120). Now if the simplest possible form of such alternative inheritance hold, viz., that the offspring resemble identically (in about equal numbers) either the one parent or the other, the frequency-distribution in the contingency-table for parent and offspring must be given by a very simple rule. One-half of the offspring of parents of any one type of character must be of the same type; the other half—if we neglect the small degree of homogamy actually existing—must be distributed similarly to parents of the other sex. A like rule, *mutatis mutandis*, would hold for brother-brother or sister-sister tables.

17. Here then we seem, at first sight, to have the clue to the overweighting of the diagonal frequencies; it may be due simply to the exclusive or alternative character of the inheritance.

When tried quantitatively, however, the theory breaks down. It gives far too great an overweighting. Thus take the figures of Table IV (condensed from Table I of Professor Pearson's Memoir, with the addition of figures for mothers from his Table III). There were 358 fathers of sons with eye-colours 1 or 2, and 269 mothers. Assuming random mating and

* 'Natural Inheritance,' pp. 138, 139.

alternative inheritance, the number of sons with eye-colours 1 or 2 to be expected is therefore $179 + \frac{269}{1000} . 179 = 227{\cdot}1$, whilst only 194 were observed.

Table IV.—Eye-colour of Father.

		1, 2	3	4	5, 6	7, 8	Total.
Eye-colour of son.	1, 2	194	70	41	9	21	335
	3	83	124	41	13	23	284
	4	25	34	55	11	12	137
	5, 6	27	12	19	24	23	105
	7, 8	29	24	24	12	50	139
Total		358	264	180	69	129	1000
Mother of sons		269	289	151	144	147	1000

Table V.—Frequencies Calculated on the Assumption of Simple Alternative Inheritance.

	1, 2	3	4	5, 6	7, 8	Total.
1, 2	227·1	35·5	24·2	9·3	17·3	313·4
3	51·7	170·1	26·0	10·0	18·6	276·4
4	27·0	19·9	103·6	5·2	9·7	165·4
5, 6	25·8	19·0	13·0	39·5	9·3	106·6
7, 8	26·3	19·4	13·2	5·1	74·0	138·0
	357·9	263·9	180·0	69·1	128·9	999·8

The number to be expected with eye-colour 3 from the same fathers is $\frac{289}{1000} . 179 = 51{\cdot}7$ against 83 actually observed. Proceeding in this way we obtain the figures of Table V, from which the observed frequencies diverge largely. It will be seen that the calculated frequencies in the diagonal compartments, corresponding to identity of eye-colour in father and son, are in every case much too great, whilst the calculated frequencies of the diagonal-borders are too small. The two or three other cases which I tried (though less completely) at random, gave similar results, and it would seem therefore that the theory of simple alternative inheritance or "exclusive inheritance without reversion" must be rejected or modified. It gives, as pointed out by Professor Pearson, a value of the correlation-coefficient approximately coincident with that observed, but the distribution of frequency it implies is not in accordance with fact. If the inheritance of eye-colour be exclusive or alternative in some way, if it involve for instance

a Mendelian separation of characters in the formation of the germ cells, the results of such process are not visible in their simplest form, but are somehow masked.

18. The anisotropy of eye-colour tables is, however, extraordinarily well marked. I have examined the 24 tables of Professor Pearson's Memoir (6, 1900) by the short 3×3-fold method (§ 5), taking the colours in the Groups 123—456—78, and the three eye-colour tables of his Huxley Lecture (9, 1903) which are of 3×3-fold form as published. Of all the 27 tables not a single one is isotropic; in 17 of the 27 the association is positive in the first and third quadrants, negative in the second and fourth. This, and not isotropy, appears to be the characteristic form; it is the distribution of signs that would be given by an excess of frequency in the diagonal compartments, though other divergences from normality may, and indeed do, occur.

19. Further, this is not the characteristic distribution of eye-colour tables alone. An extensive series of tables for inheritance of coat-colour in horses has been given by Professor Pearson and his collaborators in the 'Philosophical Transactions' and in 'Biometrika' (1, 6, 7). These I reduced to 3×3-fold form by extracting the arrays for "brown," "bay," and "chestnut," the three headings (out of 16) under which 90 per cent. or more of all the entries occur, and examined for isotropy. Of the 20 tables seven only are isotropic, whilst nine exhibit the alternate distribution of signs. Of the seven tables for relationships of the first degree, only one is isotropic whilst five are of the alternate form.

20. In Professor Pearson's Huxley Lecture (9, 1903), a number of tables were given for the resemblance of qualities between brothers and sisters, 21 of which, for 7 characters, were similarly examined. Alternation of signs was the form exhibited by the three tables given for each of the characters, curliness of hair, athletics and temper, and by two tables of the three for health.* Of the 21 distributions altogether, only 6 were isotropic, 13 exhibiting alternation of signs. Taking together the eye-colour tables, coat-colour tables, and the miscellaneous tables of the "Huxley Lecture," 68 distributions were examined, of which 13 only were isotropic, for the grouping used, whilst 39 exhibited alternation of sign. These tables include certainly the great majority of the tables for inheritance of unmeasured

* In grouping the tables in which the original classification was higher than threefold, the following were taken as the central classes:—*Health*, normally healthy; *Hair colour*, brown; *Ability*, slow, intelligent; *Handwriting*, moderate. Professor Pearson regards the *Athletics* table as unreliable for a special reason, but it appears to me to be only a marked example of the common characteristic.

characters, with a three-fold or higher classification, and based on a considerable number of observations, that have been recently published.

21. With the exception of the tables for handwriting, the distributions for all the unmeasured characters exhibit a common peculiarity, viz., a tendency to departure from isotropy due apparently to excess of frequency for identically-named characters in the two individuals. Tables for measured characters, on the other hand, exhibit no such peculiarity so far as I have investigated them by the same rough test.

Now this result seems curious. If we had to deal with two distinct modes of inheritance, we would expect a contrast between some characters and others in the case of measured variables, and between some characters and others in the case of unmeasured qualities. The contrast actually appears to lie between measured characters as a whole and unmeasured characters as a whole. The result is such as to suggest that the excess of homonymous pairs, observed only in the latter case, may be of subjective origin, arising in some way from the mode in which different observers assign names to pairs of qualities that are more or less vaguely defined.

22. As the truth of this hypothesis would gravely affect the interpretation of many recent statistics, I decided to put it to the test of experiment, and hope to be able to publish shortly a full account of the results. Here it may be briefly stated that the effect of bias and personal equation in the experimental case was of the same kind as that noted above, the tables of observers' returns exhibiting a marked excess of homonymous pairs (together with, in some cases, an excess of contrasts). A quantitative comparison, however, between the divergence of observers' returns from truth and the divergence of some of the preceding contingency-tables from normality showed that the latter was considerably the greater. The nature of this divergence demands, accordingly, further elucidation. At present it certainly cannot, owing to its magnitude, be ascribed with confidence entirely to a subjective origin, whilst on the other hand, the absence of any confirmation from statistics of measurements, and the qualitative similarity to subjective effects, compel some reserve in assuming a biological significance.

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On the Influence of Bias and of Personal Equation in Statistics of Ill-defined Qualities: An Experimental Study.

By G. Udny Yule, Newmarch Lecturer in Statistics, University College, London.

(Communicated by Professor O. Henrici, F.R.S. Received November 4,—Read December 7, 1905.)

(Abstract.)

I. This experiment was undertaken to elucidate the real character of such statistics as those of eye-colour, hair-colour, temper, health, etc., which have been given, *e.g.*, by Mr. Galton and by Professor Pearson. The statistics are, it should be noted, not merely statistics of *qualities*, but of *ill-defined qualities*, the only guidance to the use of the terms of classification being—with some exceptions—common usage. Strictly speaking we must remember that data so collected are statistics, not of qualities themselves, but of names assigned thereto. It was desired to determine how far the distinction is of importance (1) as regards the naming of single samples: (2) as regards the naming of pairs, two samples of a quality being named more or less together, by themselves, for forming a contingency-table.

A matt-surfaced photographic paper was printed by successive exposure to 16 depths of tint, from a slightly impure white to a deep blackish-brown. Small scraps of about ⅜-inch square were cut from each tint, and mounted on cards, two scraps being placed on each card, combined in such a way that every possible combination occurred, making $16 \times 16 = 256$ cards. Observers were then asked to name the tints on each card under one or more of the following schemes of classification, each observer naming the whole pack:—

Series A.—1. Light. 2. Dark.

Series B.—1. Light. 2. Medium. 3. Dark.

Series C.—1. Very light to light. 2. Rather light. 3. Medium. 4. Rather dark. 5. Dark to very dark.

The cards in the pack were arranged, by shuffling, in a more or less random order. Returns were obtained from 34 volunteer observers, who sent in 17 schedules under Series A, 20 under Series B, and 30 under Series C.

II. As regards the way in which single tints alone are named: (1) No observer, as might be expected, is quite self-consistent in his naming; (2) the inconsistencies are greater for Series B than for A, and greater for C than for B; (3) the observers attach very sensibly different meanings to the terms used for classification; (4) As a combined result of (1) and (3) the terms used for classification do not determine discrete classes, but

very widely overlapping frequency distributions. In Series B, for example, the following is the distribution of the actual tints under the names :—

Table showing Distribution of Tints under Names for the whole of the Twenty Observers who made Returns under Series B. 10,240 Observations.

Name assigned to tint.	Number of tint (1 = white, 16 = very dark brown).																
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	Total.
Light ...	640	640	636	618	521	368	269	186	93	23	15	—	—	—	—	—	4009
Medium	—	—	4	22	119	272	366	451	524	564	516	227	193	21	6	1	3286
Dark ...	—	—	—	—	—	—	5	3	23	53	109	413	447	619	634	639	2945

The result suggests that methods which treat as discrete classes determined only by names in common use are not strictly applicable, and that quantitative results obtained by such methods can be regarded only as useful illustrative analogies.

III. Contingency-tables were formed from each observer's schedules for the names assigned to the upper and lower tints on each card. If an observer returned quite without bias, the frequencies in the compartments of his table should be given by the rule of independence (total of row × total of column ÷ by whole number of observations). There proved, however, to be a distinct tendency to return an excess of pairs of the same name; this tendency, though vanishingly small for Series A, became marked for Series B, and more marked still for Series C. This feature was emphasised when different observers' results were pooled, as the pooling of results of different observers *who are quite unbiassed*, tends in itself to give an excess of homonymous pairs. In Series C there was also an excess of contrasted pairs. The following table gives the actual aggregate of returns for Series B. The first number is the theoretically correct frequency, the number after the sign the excess or deficiency of the actual returns.

Name assigned to lower tint.	Name assigned to upper tint on card.		
	Light.	Medium.	Dark.
Light	785 + 65	633 − 62	583 − 3
Medium	653 − 35	527 + 66	486 − 31
Dark	570 − 30	460 − 4	423 + 34

The above table includes returns from the 20 observers; separate tables for the first and second ten in alphabetical order gave reasonably consistent results, the frequencies of homonymous pairs being in excess in every case.

An experiment was also tried, eliminating the returns for certain cards in the pack with contrasted tints so as to make the distribution a correlated instead of an independent one. In this case the contrasted pairs returned for Series B seem to be in excess instead of in defect as above, the correlation coefficient calculated for a division between light and medium for the one tint and medium and dark for the other, being slightly lower than the true value for a similar division of the actual data. For symmetrical division the coefficient was slightly higher than the true.

IV. Certain of the contingency-tables given by Professor Pearson were examined to see how far the observed peculiarities might be due to subjective influences. The excess of homonymous pairs, and, indeed, the correlation, in the eye-colour table, for homogamy between husband and wife would seem to be largely due to such influences. In eye-colour tables for brother and brother, and for father and son, the divergencies of the distribution from normality are of the same type as the divergencies, in the above experiment, of observation from truth, but much larger. For the brother-brother tables for temper and for curliness of hair, speaking generally, the same thing holds. There is very possibly, accordingly, some real objective effect, the nature of which requires elucidating, but considerable reserve is necessary in view of the qualitative similarity to effects of subjective origin. A collection of data is required in which these are eliminated by the use of good representative scales, or possibly by the naming of the two members of a pair quite independently by different observers.

An Electrical Measuring Machine for Engineering Gauges and other Bodies.

By P. E. SHAW, B.A., D.Sc.

(Communicated by Professor J. H. Poynting, F.R.S. Received December 1, 1905,—Read January 25, 1906.)

§ I.—PRELIMINARY.

1. End standards of length, called gauges, are tested and compared with one another by means of measuring machines, made by many engineering firms in England and abroad.

These machines are identical in principle, and differ little in form from the original one invented by Sir Joseph Whitworth.

Their principle is that the gauge rests against one jaw, fixed, of the machine, whilst the other jaw is moved forward by a micrometer screw until it touches the gauge. Compression, more or less, of machine and gauge, is required before any indication can be obtained that both jaws are firmly in contact with the gauge.

These machines will in future be spoken of as mechanical-touch machines, as distinguished from the new machines called electric-touch machines. In practice it has been found desirable to introduce some index of the touching—*e.g.*, a gravity feeler, spirit level, or the raising of liquid in a fine tube.

2. The weight of the gauge, often large, should never, as is sometimes done, be supported by the grip of the two jaws; for then there is considerable end-thrust of the jaws with consequent longitudinal strain in the machine, in addition to the vertical strain due to the weight of the gauge. If the whole or part of the weight of the gauge be supported by the jaws, though the latter be parallel to one another initially, they will not be so after being weighted.

The errors which enter into the measurements made by these machines differ in kind and degree according to the nature of the gauge. There are three kinds of gauge.

A. *Bar Gauges with Plain Parallel Ends.*—Each jaw has a flat face and each end of the gauge has a flat face. Each of the four faces has defects in planeness, and in being not strictly parallel to the other three. If in addition a gravity feeler be used, it introduces extra errors. To admit so many errors of unknown amount in the measurement of gauges may be permissible in present-day engineering trade practice, but it does not satisfy the

demands of exact metrology. Thus to obtain accuracy, surface contact should be abandoned and point contact used, and these gauges should be measured between two rounded points or spheres, in which case no assumptions are made as to the perfectness or parallelism of the surfaces involved.

For such point contact we require a more delicate means of perceiving contact than the mechanical one; hence the electric-touch method is employed. It has been developed by the writer in a series of researches since 1900.*

B. *Cylindrical Gauges.*—The flat faces of the jaws touch the cylinder with two line contacts at opposite ends of a diameter of the cylinder. Non-planeness and non-parallelism of the jaw faces, as also imperfections in the cylinder, introduce errors, though these are less serious than for Class A. Thus, for accuracy, line contact should be superseded by point contact, the measurement to be made between lines or edges on the jaws, the lines being not parallel with the axis of the cylinder.

C. *Sphere or Bar with Spherical Ends.*—The face of each jaw touches the sphere at a point. Since the jaw faces are imperfect, error can only be avoided by providing that the contact of the two faces with the sphere always occurs at the same places. Thus, for accuracy, contact may be made, as usual, between flat faces of the jaws, if the surfaces be made true and contact be always at the same points on the flat faces.

From these remarks it appears that, for each kind of gauge, measurement should be made by point contact, the jaws to support no part of the weight of the gauge, and the end thrust on them to be reduced as much as possible. The method described below fulfils these conditions, and has in addition the advantage of being more sensitive than the old method.

§ II.—Description.

1. The first machine made was derived from a much less sensitive instrument described previously.† A massive bed and pillar carry a bracket, in which works a vertical micrometer screw with divided head and vernier. The nut is double, and has a tightening spring between the two parts. The screw shaft carries a pulley of the same diameter as the divided head (6 inches). The top of the screw shaft passes thoroughfare through a plate rigidly screwed to the bracket, the plate serving to steady the axis of the screw. A heavy pulley stand has a string passing round its three pulleys

* See 'Phil. Mag.,' December, 1900, and March, 1901; 'Electrician,' March, 1900, and March, 1901; 'Roy. Soc. Proc.,' 1903, 1904 and 1905; 'Nature,' September, 1905.

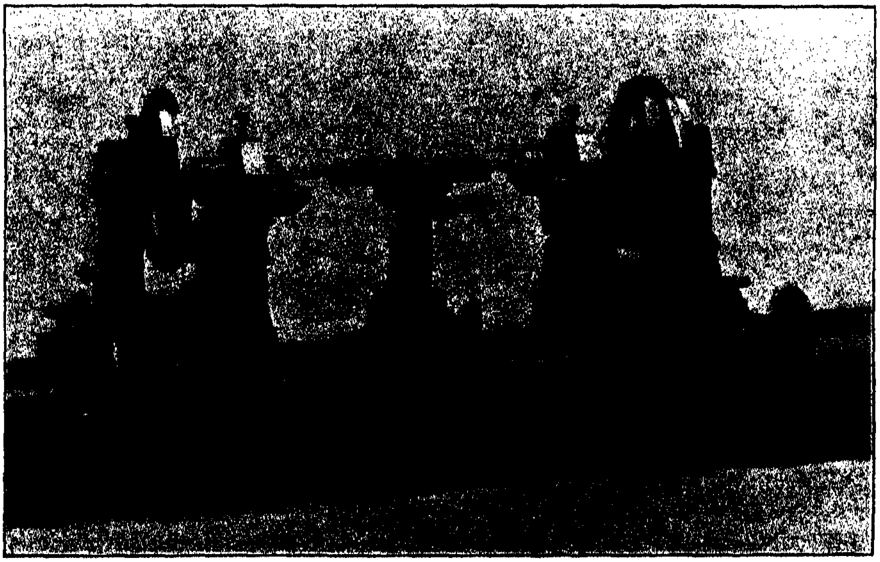
† Shaw, 'Phys. Rev.,' March, 1903.

and going to the large pulley on the screw, so that the observer works the pulley handle instead of handling the screw direct. The advantages of not handling the screw system during a measurement are obvious.

The gauge being on the glass plate and under the screw point, the screw is rotated, and when its top point touches the top point of the gauge an electric circuit is completed and a telephone sounds, as it does whenever the screw point touches or leaves the gauge.

To find whether the gauge is uniform, one point after another on its top surface is brought below the screw point, and the micrometer is read for each place; the gauge is reversed, the former top face now resting on the glass surface, and readings are taken at various places as before.

Whilst these processes are difficult for a cylinder or sphere, they are inaccurate for bar gauges, since the end of the gauge on which it rests is more or less convex; the gauge does not rest firm on the glass and errors arise. Experience with this instrument showed that, while retaining point contact, the electrical touch, and the pulley system of working, we ought to support the gauge in such way as to leave both surfaces free, so that electric contact can be made on each surface.



2. The latest form is shown in accompanying figure. In general appearance it is somewhat like the usual mechanical-touch machines mentioned in § I.

There are two headstocks and a table in the centre, all resting on a massive cast-iron bed. The headstocks each carry micrometer screws and nuts with

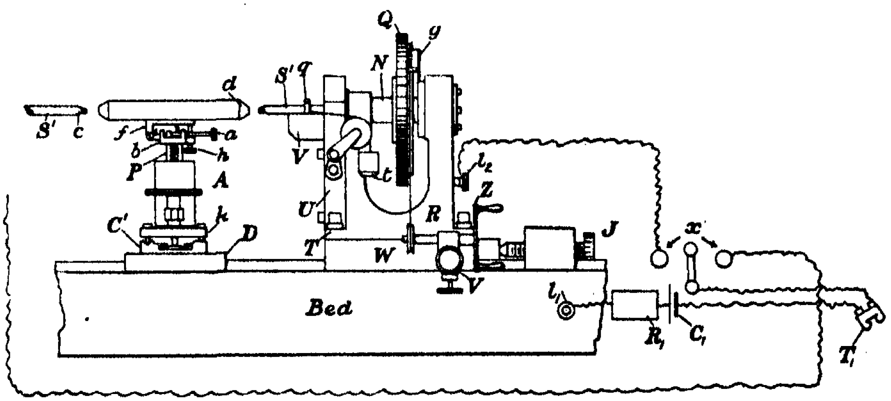
graduated heads, and these measure the gauge, which rests on and is clamped to the table. The gauge being clamped on the table, is set true with respect to the axes of the micrometer screws, by adjustments of the table.

The left screw point is brought into electric contact with the gauge; then the right screw point is brought into electric contact with the gauge, and when the current passes through the gauge from one measuring point to the other, the two divided heads are read.

To turn the graduated heads the screw system is not actually touched by hand, but is worked by a hand pulley and string, the former being attached to the base of the headstock, there being a large pulley on and concentric with the head. Details are shown in figs. 1, 2, and 3.

The left headstock is a replica of the right headstock; to save space, only the measuring point of the left headstock is shown.

Fig. 1.



Gauge d rests on the table f and is clamped to it. The table top can be brought into any desired plane by two rotations, as follows:—

3. *Rotation about a Vertical Axis.*—The pillar P carries a bracket b to which is fitted an adjusting screw a. The top of the pillar is turned cylindrical and fitting this cylinder is a collar which carries an arm. A spring attached to arm and bracket keeps the latter always in contact with the end of the horizontal screw a. Thus the table top and gauge can be rotated either way by a small angle.

4. *Rotation about a Horizontal Axis across the Bed.*—The bracket b has a hinge on the left side, the top plate f hinging there and being held down to adjusting screw h by a spring attached to the bracket below and to the plate above. The vertical screw h works in the bracket. Rotation either way is thus provided.

Besides the two rotations, the table has three translatory movements:—

5. *Translation Horizontally along the Bed.*—The table touches the bed at five places by studs let into base D of the table. The table can be placed anywhere along the bed and clamped. Plate D is grooved to fit part X of the bed (fig. 2) and touches it by five studs, four on X and one on Y.

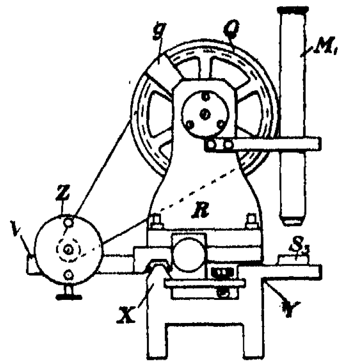


Fig. 2.

6. *Translation Horizontally across the Bed.*—Above plate D is a plate C', ploughed out from front to back in a V-groove as shown on the left side (fig. 1); the slide k which carries column A has three levelling screws, two resting in the V-groove and one on the plane on the right. A spring is arranged to keep the slide pressing firmly on its feet, so that it has freedom only in a horizontal line across the bed.

7. *Translation Vertically Up and Down.*—The column A is hollow and can slide up and down outside a pillar carried by k; the pillar has a triangular cross-section, and the fit between column and pillar can be adjusted by four screws not shown. The total weight of column, table and gauge is balanced by a lever with counterpoise weight on it. The fulcrum of the lever can be set at any place desired on a separate pillar. None of these counterpoising arrangements are shown in fig. 1. In order to raise or lower the gauge, the observer works the lever up or down on its fulcrum, this slide motion being very free.

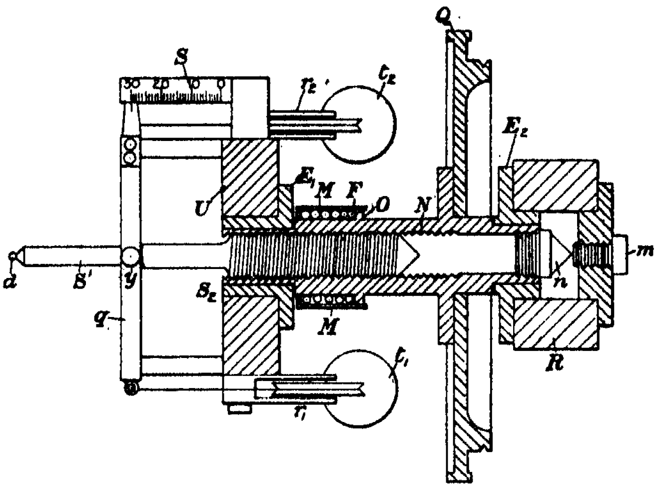
Both the two latter translations have millimetre scales attached, so that the amount of any translation can be known. Also, after the table is moved, it and the gauge on it can be brought back accurately to any desired place.

8. The five adjustments mentioned are sufficient for setting the gauge true. As will be seen later the rotations take some time to perform, but they are seldom required; the translations, being slides, are quickly performed. These slide movements are quite satisfactory. Screw movements and then rack and pinion movements were tried and found defective; the first being slow and the second irregular.

In figs. 1 and 3 is shown a nut N, working in bearings E_1, E_2 and having the screw S in it. A steel cone n is screwed into the right end of nut N, and bears against the stop m. There is a helical spring F which presses forward against bearing E_1 and back against the nut forcing the cone n against stop m. The point of n lies truly in the axis of nut N and the front face of m is ground truly plane and by a special device is made accurately normal to the

axis of the nut N. By this means when the nut is turned it should have a true rotational motion, without any periodic to-and-fro translation along its axis, such as occurs with the usual coned or flat bearings. Subsequent calibration of the screw shows how nearly this ideal has been approached.

FIG. 3.



Fixed to nut N is a graduated wheel Q and a double vernier g is shown attached to the casting R. The wheel is centred true with the nut axis by means of the double vernier in the usual way.

The casting R has a front part T to which an upright plate U is screwed. This plate carries the bearing E_1, bracket V on which runs the yoke q and the pulleys r_1, r_2 (fig. 3).

9. The screw S' would rotate with the nut if free, but as the yoke q which runs on the bracket V is clamped to the screw, the latter acquires a simple translatory movement along its axis, in or out of the nut, according as the latter rotates right- or left-handedly. The screw spindle carries an index mark by which the position of the screw in the nut can be seen on the fixed scale S (fig. 3).

In this micrometer system of rotating nut and translating screw, it is essential for accuracy that the nut have no translation and that the screw have no rotation. The former condition should be achieved by the cone end method of working (see para. 8 above); the latter by making yoke and bracket rigid and ensuring by a weight that the former presses the latter with constant force.

Backlash and looseness between micrometer screw and nut are minimised

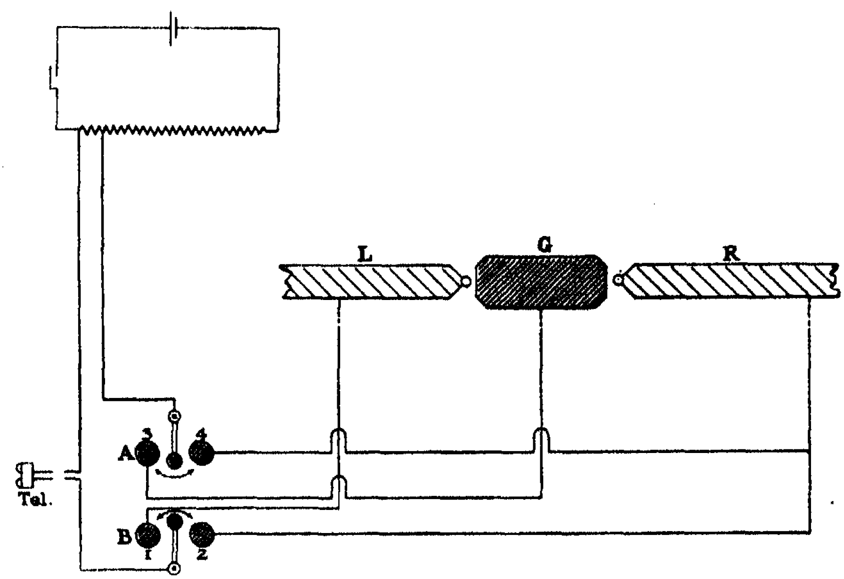
by the pull of weights t_1 and t_2 (fig. 3) from which pulley strings pass over pulleys r_1 and r_2 to the yoke ends. By this means the screw is pulled back into the nut by a steady force.

10. The casting *R* is electrically insulated from the plate *W* by a mica sheet and by having the screws which bind *R* and *W* bushed with ebonite. The base of plate *W* is grooved to fit the part *X* of the bed and presses that part by four studs; one other stud presses on the flat part Y. Thus the headstock rests firmly on the bed at five points of support and has only one degree of freedom, along the length of the bed.

Fixed on the side of *W* is shown a bar *V* on which the hand pulley *Z* can be pulled forward and clamped, so as to take up slack in the pulley cord.

11. The two micrometer screws and nuts are cut and ground with great care on the plan adopted by H. A. Rowland,* and when examined under the microscope the screws appeared highly polished and regular. The calibration, described below, shows that there is a small periodic movement at each rotation of the nut, probably due to its bearings being excentric with respect to its axis. But this movement is perpendicular to the line of measurement and the resultant errors in the micrometry are of a lower order and probably negligible.

FIG. 4.



The screw-threads are about 2 inches and the nuts about 4 inches long, so that the screws do not leave the massive nuts at any point in the run.

* See article on "Screw," 'Ency. Brit.'

Steadiness in temperature of the screw is thus obtained. The screw diameter is 1 cm., the pitch ½ mm., the graduated head has 500 divisions, and the vernier reads tenths; so one vernier division corresponds to 1/10000 mm. (or 0·1 μ) in the micrometry.

The bed is 5 feet long and weighs 200 lbs.; it rests on three studs, two 1 foot apart under one neutral line and one under the other neutral line.

A voltaic circuit consisting of a cell C_1, a resistance box R_1, switch x, and a telephone T_1, are joined to binding screw l_1 on the bed and to another binding screw l_2 on each headstock. The switch is put to right or left according as one wishes to make contact between gauge and left screw or between gauge and right screw.

A better arrangement of circuit is to use two mercury throw-over switches A, B, with the circuit as shown (fig. 4). On the right is the gauge G, with the measuring ends R and L of the micrometers shown. To make contacts, use connections as shown in the table below:—

Contact.	Switches.
L, G	1 and 3
R, G	2 and 3
L, G and G, R	1 and 4
L, R	1 and 4

The micrometer microscope *Mi* rigidly mounted on the right headstock (fig. 2) is used for reading the standard invar scale S_3. By this means, as in the Pratt and Whitney measuring machine, the end standards may be compared with line standards of length.

§ III.—Materials Used.

The bed is of cast iron. Most of each headstock is of cast iron in one piece (see illustration, p. 342), but the front plates and brackets and the base plates of headstocks and table are of wrought iron. Brass is used for the nut bearings and for most of the table. The micrometer screws are of silver steel, and the micrometer nuts of bell-metal, which is very hard.

The part of the screw spindle projecting from the front of the nut is of first grade invar. All caps and fittings on the screw ends are also of invar. Since invar cannot be ground very true, the faces of the caps are of thin steel. The idea is that these projecting parts, being of small dimensions and necessarily uncovered, are more liable to temperature changes. The

consequent errors in the micrometry are small, since the expansibility of invar is so minute.

The terminal points of the screw spindle are beads of iridio-platinum, which, being hard and non-oxidisable, is the best substance for electric-contact work. The beads are continuous, with a short piece of iridio-platinum wire, which was hammered into a hole drilled in the end of the invar.

§ IV.—Adjustments and Tests.

Before the machine is used for actual measurement many adjustments have to be made to ensure accurate working. In this preliminary work defects in design or make can be detected and rectified.

1. *Testing the Planeness of the Bed and Setting one Screw Axis Parallel to the Bed.*—There are three surfaces on the bed on which rest the studs of the uprights. Two surfaces form an inverted V, and the other surface is horizontal (fig. 2). The edge formed by the first two surfaces should be parallel to some third straight line in the third surface. Place a card on the invar end of the left screw, remove the right headstock, and place a microscope with axis roughly parallel to the bed. Set the cross-hairs of the microscope on some very small object on the card; run the headstock along the bed about 1 inch, and work the screw back till the small object is again in focus. If the image does not come exactly to the cross-hairs, the studs at the base of the headstock are screwed in or out till this condition is obtained. These studs fit specially tightly in the headstock, and are adjusted by a special spanner.

Repeat the above double operation in various parts of the bed; if satisfaction is obtained the right micrometer screw axis is parallel to the ridge formed by the Λ surfaces, and the three bed surfaces are sufficiently nearly plane.

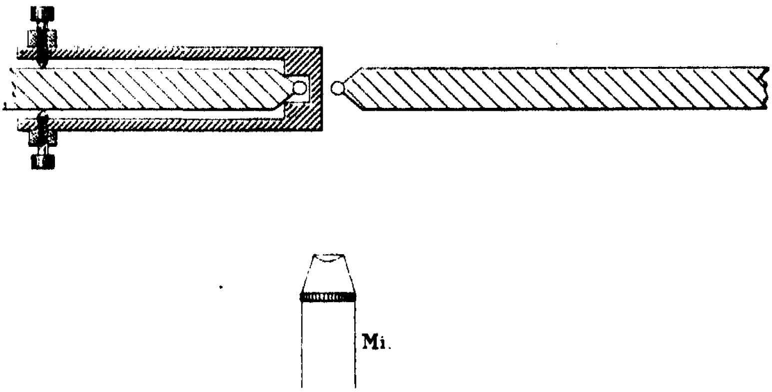
2. *Setting the Two Screw Axes Collinear and Arranging that the Bead Ends Meet at their Extreme Points.*—First set the right screw axis parallel to the bed as above; next run one headstock along the bed until the bead ends meet. Set a microscope up horizontal with its axis perpendicular to the bed and focus it on the contact of the bead ends.

Arrange the electric circuit so that the telephone sounds when the beads touch. If, when the beads are seen to meet in the microscope, they are also heard to meet by the telephone, the adjustment is complete. The studs at the base of the headstock are screwed in or out till the best result is obtained.

The microscope must next be placed with axis vertical so as to look down

on the contact from above and the adjustment repeated. In performing this adjustment the two studs on the same side of the Λ must be screwed in equally so that the screw axis is moved parallel to itself across the bed; if this be not done, the former adjustment will be destroyed.

FIG. 5.



Under the most favourable conditions it is found that the telephone sounds slightly after the beads appear to meet, the distance moved on by the screw end being about 0·5 μ. It was found by putting an end-cap on one side (fig. 5), and viewing with a microscope again, that the same lag, 0·5 μ, occurs as before. Hence this amount of movement is required to complete electric touch after the surfaces appear to be in contact.

Unless the bead ends are set carefully, as above, a constant + error will occur in measuring the actual gauge length by a line standard.

3. *Testing for Backlash of Micrometer Screw.*—A steady gravity pull is arranged to keep the screw back in the nut. On trial, backlash is found to be small, and need not be considered (except in the calibration work), for readings are taken at both ends when in contact with the gauge, and the end pressures suffice to eliminate looseness.

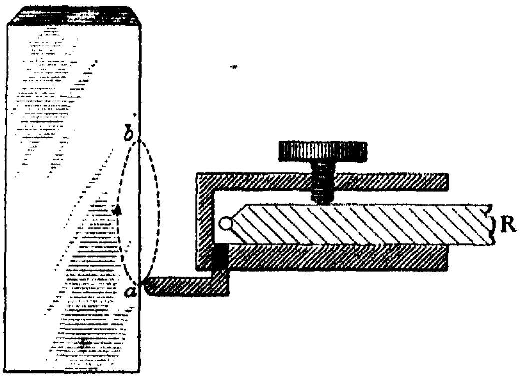
4. *Setting the Two Translations of the Table Perpendicular to the Bed.*—As will be shown later, it is possible to make a cylindrical gauge very true both in straightness of axis and constancy of diameter.

Having tested a cylinder and found it nearly perfect, it can be used as follows for setting the table. Mount it on the table with its axis horizontal and across the bed. Two processes follow:—

(*a*) Set the gauge perpendicular to a nut axis. (*b*) Set the translation of the table parallel with the gauge axis.

(*a*) Mount on the right screw end, *R*, a cap, having a projecting excentric point (fig. 6). Unclamp the yoke from the screw, so that on rotating the

Fig. 6.



nut, the screw spindle turns with it and the excentric point describes a circle perfectly normal to the nut axis. Rotate the gauge on a vertical axis by the rotating screw of the table, until the excentric point just touches it, as shown by the telephone, on passing at points *a*, *b*. The gauge axis is now perpendicular to the screw axis.

(*b*) Remove the cap with excentric and clamp the yoke to the screw, care being taken to make the linear scale and divided head scales agree. Bring up one bead end to make contact with the cylinder, the table and gauge being moved up and down so that the latter makes bare contact in passing. Read the micrometer head. Run the table and gauge across the bed to various positions, making contact and taking readings. The cylinder being true, we can at once see if the translatory movement of the table is straight and perpendicular to the bed. Set the line of this run parallel to the gauge axis, by adjusting the studs at the base of the table until the micrometer readings agree at the ends. This throws out adjustment (*a*)—so (*a*) and (*b*) are repeated alternately till each is completed. If intermediate readings are different from those at the ends, the V-groove at the base of the table is not straight. In the measurement of a cylinder later (*see* Table III) the non-straightness of the groove is shown by irregularity in Column A. In that table two consecutive readings 5 mm. apart differ by 10 μ, at most thus the angular displacement of the gauge is 1/500 radian = 7′ and versin 7′ = 0·000002.

Thus the error produced, due to non-straightness of the groove, in measuring this gauge is, at worst, 1/500000 × 20 mm. or less than 2/5 of a vernier division, which is negligible here; the error produced would only

be important for a long gauge. The groove should be ground until it satisfies the above tests better.

Having adjusted the cross horizontal translation the vertical translation can be done in like manner. The gauge is put on the table, with axis vertical, the excentric point and the rotating screw being used until the gauge is perpendicular to the screw axis; the rest of the adjustment is obvious, the three levelling screws being used for adjusting the column.

5. *Testing for Flexure of the Bed.*—In finding the actual length of a gauge, one headstock is moved, causing change in the flexure of the bed. (In ordinary comparative work of testing and comparing gauges, the headstocks are stationary, and no error, due to this cause, arises.) Put a plane-faced gauge on the table. Bring up each screw end to touch its faces. Read one screw head, say the left, and then move the right headstock away on the bed to a definite distance. Any change in the contact reading of the left end is due to change in flexure of the bed. From a series of such observations the error for any movement can be allowed for. In the present apparatus it amounts to 3 μ in some cases, the bed not being strong enough for the heavy headstocks. But little error can enter into the micrometry, for the correction to be made in any case is known.

§ V.—Calibration of Micrometer Screws.

Each screw is tested in two ways, (*a*) for long run, by comparison with the units on a line standard of length, (*b*) for short run, by interference bands:—

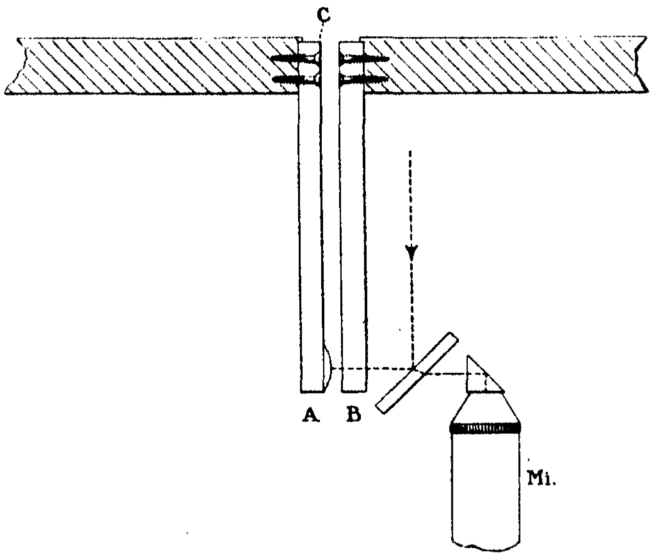
(*a*) An invar standard metre, made by the Société Genevoise, is read by a microscope on the right headstock (fig. 2). The headstock is given small movements along the bed by a jockey screw in the usual way.

On the end of the left micrometer screw is placed a cap with a plane face as in fig. 5. Bring the end of the right screw into electric contact with this cap. Set the microscope cross-wires on a line of the scale, and by the jockey screw move the right headstock back 1 mm. of the scale, and run the micrometer screw forward, by rotating the nut, to make contact again. The error in 1 mm. run of the screw as compared with 1 mm. of the scale is thus found. The process is reversed and then repeated at any part of the two screws. It is found that the probable error of one microscope reading is about 0·3 μ, whereas the probable error in making the electric contact is about 0·05 μ.

The tables of results need not be given. The average run per mm. for 7 mm. of the right screw was found to be 1·0003 mm.; for the left screw the result was 1·0002 mm. The line standard is here taken to be correct. The above process gives us the average pitch of the screw and also the

behaviour of the screw in different places. In an attempt to make a perfect screw it would be a means of finding whether or not the screw and nut are in accord. Thus, in this machine, both right and left screws give distinctly larger runs for every second millimetre. For instance, the right screw has the following values for consecutive mm.: 1·0001 mm., 1·0018 mm., 0·9979 mm., 1·0011 mm., 0·9991 mm., 1·0017 mm. By separate measurement it can be shown that this periodic error is not due to the line standard.

FIG. 7.



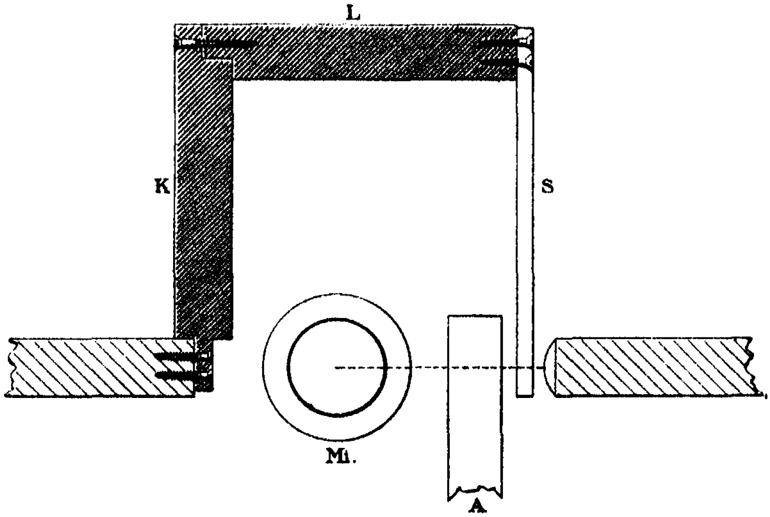
(*b*) Sodium light is used from a bunsen burner, the heat of which is carried away by a funnel and stove piping. On each screw end is fixed a cap with plane end, to which is screwed a microscope slide. On one slide A is a small lens (fig. 7) mounted by Canada balsam. The sodium light falls on a glass slide at 45 degrees, and is reflected to the microscope as shown in the figure (which is a plan). The Newton's rings formed between the lens and slide B_1 are caused to open from or close to their centre according as one screw end is moved to or from the other screw end.

This plan of calibration is not satisfactory if the screw or nut be bent or "drunk," since then the plate *B* moves to and from *A* at each rotation in a periodic way. But the method indicates any such defects in screw and nut, however small. Curve I (fig. 9) obtained thus shows that the right screw-nut system acts as if bent. From the curve we can find the extent of this rhythmic error. The length *AC* (fig. 7) is 50 mm., the to-and-fro movement

on the curve is 15 μ. The angular movement of the screw in this to-and-fro movement is 15/50000 radians = 60″.

The length of screw spindle from the bead-end to the nut is 100 mm., so the diameter of the circle described by the screw point is 30 μ. This movement, being perpendicular to the line of measurement, is immaterial.

FIG. 8.



To avoid any such to-and-fro movement, and obtain the true screw calibration—the plan adopted is to have the interference produced at a place on the axes of the screws as in the elevation (fig. 8). The left screw end has a rod of invar *K* screwed to it; another invar rod *L* is screwed to *K*. On the end of *L* is screwed a microscope slide. The right screw-end cap has a small lens mounted on it. Set up the slide *A* to reflect sodium light, view with the microscope *Mi* the Newton's rings formed between *S* and the lens surface.

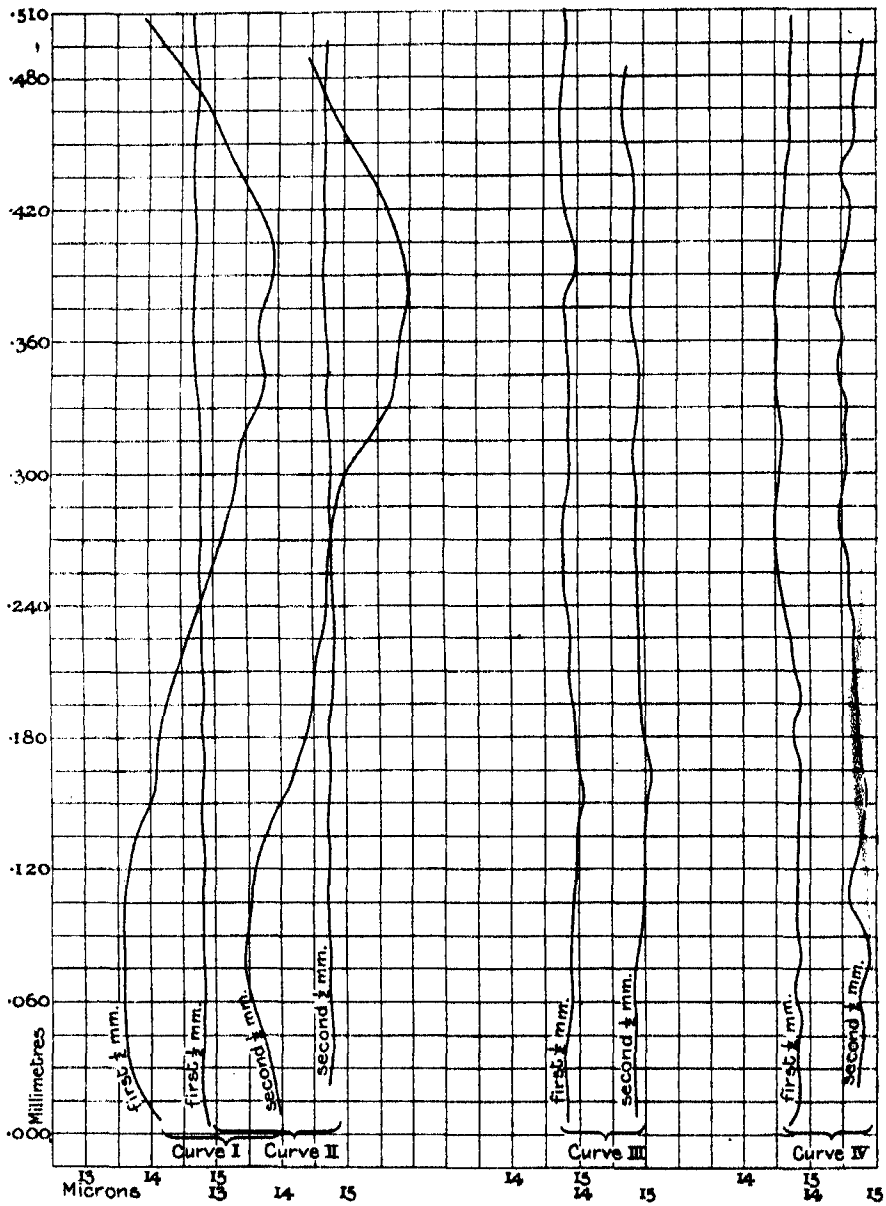
The micrometer scale is read for every 50 rings passing the cross-wires (the length traversed is thus 14·73 μ, taking $\lambda = 0{\cdot}589\ \mu$).

The interfering surfaces are never more than 200 half wave-lengths apart—so that the rings are quite clear always. Also, as the surfaces are so near, the number 0·589 μ may be taken without considering the exact values for the D_1 and D_2 sodium lines.

As soon as 200 bands have been read, the left screw is advanced to bring the interfering surfaces into contact again. Then proceed as before.

Curve II is the result, in which the periodic error of Curve I has vanished. Curves I and II refer to position 25—26 mm. on the right screw.

Fig. 9.



Curves III and IV refer to 15—16 mm. and 25—26 mm. respectively on the left screw. The scale of abscissæ is an open one, so that plotting in this way is a very severe test of a screw,

Each millimetre requires about 80 readings, each of 50 bands. Where a

curve looks irregular, readings are repeated, but in nearly all cases the readings were found correct to 0·05 μ. The millimetre of the screw corresponding to Curve II is more regular than any other tested, the length of run of the screw for 50 bands lying in all the 80 cases between the narrow limits 14·72 μ to 14·90 μ.

These calibration readings are very exact, and can almost always be repeated to $\lambda/20 = 0{\cdot}03$ micron. Higher accuracy could be attained by special precautions, but gauge work does not call for it.

Some precautions taken in this interference work are:—(1) Leave the apparatus with sodium light going half an hour before taking readings, to allow the bands to become steady. (2) Interpose several thick wooden screens between burner and machine, allowing only a narrow line of light to fall on the surfaces. (3) Avoid backlash by working only one way before a reading. (4) Count rings by twos, thus reducing the eye-fatigue by nearly one-half. (5) The invar and glass surfaces and two divided circles are the only parts not covered by thick layers of felt. (6) The angle of incidence of the light on the interfering surfaces must be 90°, within 2°, in which case the error in the readings is less than 1/1000 in the value of λ, which is immaterial if the results are not used in a cumulative way.

It should be noticed that it is impossible to have error due to a miscount of bands in such an accurate apparatus as this, since one count never differs from the next by more than 0·1 μ, generally much less, whereas error of one band would produce a sudden change of 0·3 μ and would be at once detected.

§ VI.—Applications and Results.

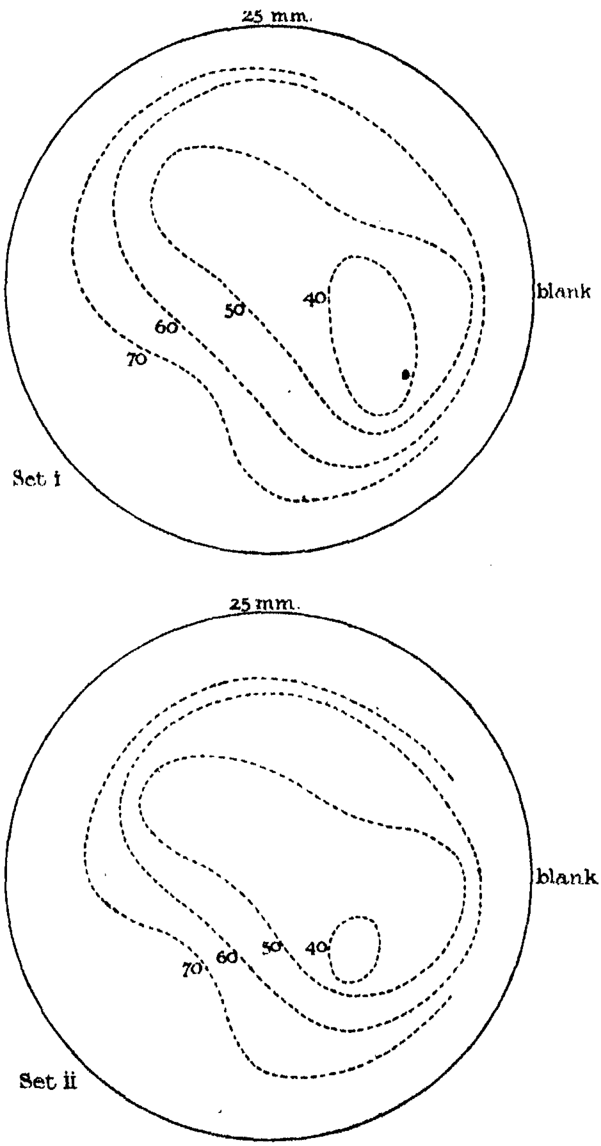
The method of connecting the electric circuit differs according to the object to be tested. In § II something has been said on this point.

1. *Bar Gauges with Flat Ends.*—These are measured between two points. The gauge is put on the table and clamped. It must first be set so as to have one face normal to a screw axis. If after performing the adjustment described in § II the two translatory movements are found to be straight lines, the gauge can be set by them. If not, the excentric movement is used for each gauge.

As will be seen from the contour curves following, the flat ends are never true planes, but for the above purpose the gauge is taken to be set when the contact readings on all points on a circle near the edge of the face are identical.

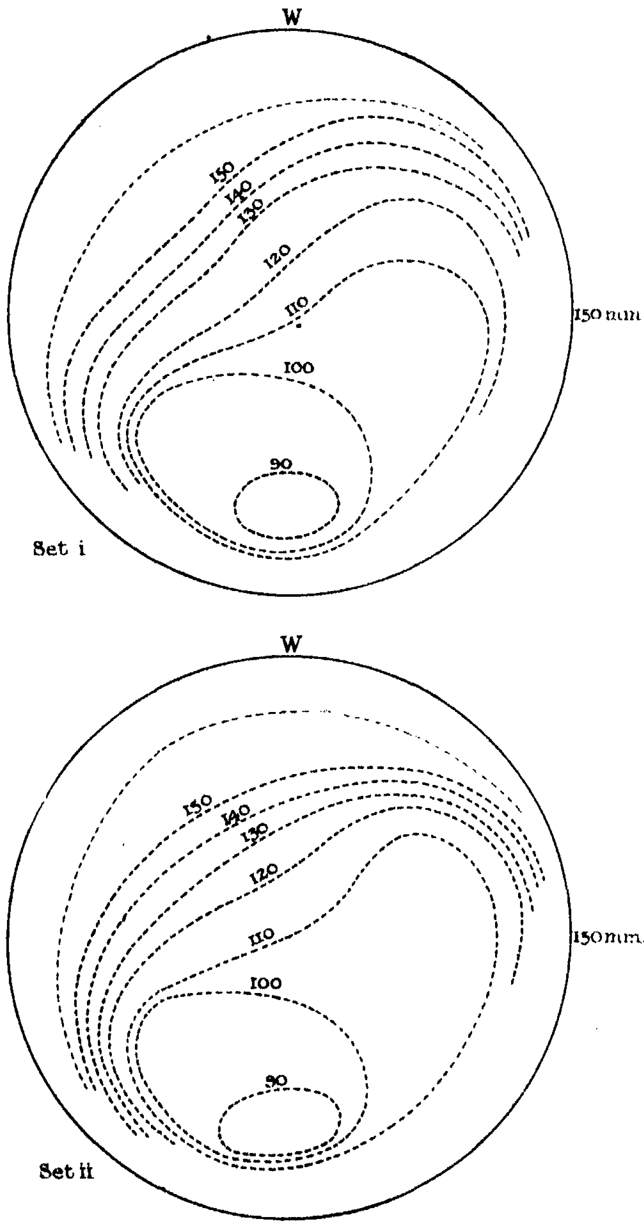
The flat faces are roughly 6 mm. across, the left screw point is brought into electric touch with the gauge at the centre of the left face, marked

1 in column A (Tables I and II). The right screw point is then brought up to touch the right face, the circuit being now arranged to pass from the left screw through the gauge to the right screw. The two micrometers are read as in columns B and C. Then the table and the gauge with it is moved 1 mm. to the left, the contact is now made at place 2 in column A. The micrometer head readings are taken and entered in columns B and C as



25-mm. Contours.

before. The whole face, except near the edge, is thus tested in 13 symmetrical places.



150-mm. Contours.

Results follow for two new gauges, made by the best firms, in Tables I, II. The numbers in columns B, C, and in other tables below, are those read

on the micrometer heads, so that they give only comparative values from place to place, and would have to be all changed alike to represent the absolute values of the length of the gauge. Column D is the sum of columns B and C; the larger the sum the less is the thickness of the gauge on the line in question. The small disagreement between the results for the two sets is shown in column Δ. The gauge is well covered throughout the readings.

For the 25 mm. gauge, Table I, the errors arising for any one position lie between 0 and 0·4 μ. These errors can be attributed mostly to bad polish of the surfaces. Grooves are visible and small changes in setting the surfaces would bring the measuring point now over a ridge, now over a hollow.

The errors in reading the micrometer can be ignored, for if any observation be repeated before the gauge be moved, the difference in the reading never amounts to more than 0·1 μ, generally less. Thermal expansion may produce a small deformation, since about ¾ hour elapses between a reading in Set i and the corresponding one in Set ii.

The differences in the gauge thickness in different places amount to 3·6 μ. To show the nature of the gauge, contour figures are drawn, one for each set. These curves are not contours for one surface in the usual way, but represent the joint effect of the two end faces of the gauge.

Table I.—25 mm. Gauge.

A.	Set i.			Set ii.			Δ = D′ − D.
	B.	C.	D = B + C.	B′.	C′.	D′ = B′ + C′.	
1	116·5	147·9	264·4	117·6	146·9	264·5	+0·1
2	117·5	146·3	263·8	117·4	146·7	264·1	+0·3
3	117·3	146·9	264·2	118·2	146·3	264·5	+0·3
4	117·9	147·7	265·6	118·7	146·9	265·6	0·0
5	117·0	140·9	266·9	118·0	148·9	266·9	0·0
6	117·0	147·4	264·4	117·5	147·1	264·6	+0·2
7	117·8	147·6	265·4	118·4	147·3	265·7	+0·2
8	166·6	147·5	264·1	117·9	146·6	264·5	+0·4
9	118·0	147·5	265·5	118·1	147·7	265·8	+0·3
10	118·0	147·5	265·5	117·7	147·9	265·6	+0·1
11	117·7	146·0	263·7	117·3	146·6	263·9	+0·2
12	118·9	148·5	267·4	119·5	147·9	267·4	0·0
18	119·0	147·5	266·5	119·6	147·1	266·7	+0·2

The units are microns ($\frac{1}{1000}$ mm.).

In the 150 mm. gauge differences are slightly more, −0·1 μ to −0·5 μ (thermal expansion exercises more influence for long gauges) than for the 25 mm. gauge.

Table II.—150 mm. Gauge.

A.	Set i.			Set ii.			Δ = D′−D.
	B.	C.	D = B + C.	B′.	C′.	D′ = B′ + C′.	
1	395·0	96·2	491·2	395·0	96·0	491·0	−0·2
2	397·0	93·8	490·8	396·0	94·5	490·5	−0·3
3	396·0	94·9	490·9	395·0	95·5	490·5	−0·4
4	398·7	93·5	492·2	398·0	93·9	491·9	−0·3
5	—	—	—	—	—	—	—
6	394·0	98·7	492·7	396·0	96·6	492·6	−0·1
7	396·0	95·5	491·5	396·0	95·0	491·0	−0·5
8	398·3	96·7	495·0	399·0	95·5	494·5	−0·5
9	—	—	—	—	—	—	—
10	396·0	93·7	489·7	396·0	93·4	489·4	−0·3
11	397·0	93·4	490·4	397·5	92·8	490·3	−0·1
12	398·0	91·7	489·7	399·0	90·5	489·5	−0·2
13	396·0	92·9	488·9	396·0	92·7	488·7	−0·2

The units are microns.

But differences in the gauge thickness from place to place amount to $5{\cdot}8\,\mu$. The contour figures are given. The faces are not normal to the length of the gauge, and no readings can be taken at places 5, 9; when in these positions one screw end touches the gauge, the other screw end does not make contact on the other face, but, if continued, would meet the gauge on its side

The actual length of this gauge was found to be, at 18°·5 C., 150·0332 mm. and 150·0325 mm. in two distinct evaluations, taken at place 1, Table II.

2. *Cylindrical Gauges.*—These are measured between lines or edges. The screw ends have cylinders or edges mounted perpendicular to the screw axes. A preferable method is to work between the screw points and to move the table carrying the gauge up and down past the screw points, so as just to establish electric touch in passing. In this way the straight lines mentioned above are virtual lines due to the passage of the measuring points past the gauge.

Before commencing measurement the axis of the gauge is set perpendicular to the screw axes. Put the gauge on the table, its axis being horizontal and across the length of the bed; take a contact between the left screw end and one end of the gauge. Move the gauge by the table till its other end makes contact with the left screw end. If the two contacts have identical micrometer readings the gauge is set.

The results for a $\frac{3}{4}$-inch gauge are shown in Table III.

Readings are made along the gauge at six places from 0·5 cm. to 3 cm. from one end. After one set of six readings the gauge is rotated on its axis to positions 45°, 90°, 135°, 180° from the original one, and for each position six readings are taken as before.

Set i takes about three-quarters of an hour and is completed before Set ii is commenced; corresponding measurements are in one line. In no cases do the result of the two sets differ by more than $\pm 0{\cdot}2\,\mu$.

The gauge is a very good one; the differences in various places are not more than $0{\cdot}7\,\mu$, though there is a distinct tapering in every position, the handle end being thickest. There is a slightly different angle of taper in the different positions. The 180° position results are almost identical with those for the 0° position, as they ought to be.

Table III.—$\frac{3}{4}$-inch Cylindrical Gauge.

	Centimetres from end.	Set i.			Set ii.			Δ = D′ − D.
		A.	B.	D = A + B.	A′.	B′.	D′ = A′ + B′.	
0° ...	0·5	236·9	197·8	434·7	233·5	201·2	434·7	0·0
	1·0	242·0	192·6	434·6	238·0	196·7	434·7	+0·1
	1·5	241·1	193·5	434·6	238·0	196·7	434·7	+0·1
	2·0	234·4	199·8	434·2	231·0	203·1	434·1	−0·1
	2·5	226·6	207·6	434·2	223·0	211·2	434·2	0·0
	3·0	235·3	199·0	434·3	231·0	204·2	434·2	−0·1
45° ...	0·5	237·6	197·0	434·6	231·0	203·6	434·6	0·0
	1·0	243·0	191·7	434·7	238·0	196·5	434·5	−0·2
	1·5	241·0	193·4	434·4	236·5	197·8	434·3	−0·1
	2·0	234·0	200·2	434·2	230·0	204·0	434·0	−0·2
	2·5	220·0	208·3	434·3	223·0	211·2	434·2	−0·1
	3·0	233·0	201·3	434·3	230·0	204·1	434·1	−0·2
90° ...	0·5	234·0	200·6	434·6	230·0	204·6	434·6	0·0
	1·0	240·0	294·4	434·4	236·0	198·5	434·5	+0·1
	1·5	240·0	194·3	434·3	237·0	197·5	434·5	+0·2
	2·0	233·5	200·8	434·3	230·0	204·2	434·2	−0·1
	2·5	225·0	209·1	434·1	223·0	211·2	434·2	+0·1
	3·0	233·0	201·1	434·1	229·0	205·2	434·2	+0·1
135° ...	0·5	234·0	200·5	434·5	231·0	203·4	434·4	−0·1
	1·0	240·0	194·6	434·6	238·0	196·5	434·5	−0·1
	1·5	240·0	194·3	434·3	236·0	198·3	434·3	0·0
	2·0	233·0	201·0	434·0	229·0	205·2	434·2	+0·2
	2·5	225·0	209·2	434·2	221·0	213·3	434·3	+0·1
	3·0	234·0	200·3	434·3	229·0	205·2	434·2	−0·1
180° ...	0·5	235·0	199·7	434·7	225·0	202·6	434·6	−0·1
	1·0	237·8	196·8	434·6	238·0	196·5	434·5	−0·1
	1·5	239·0	195·4	434·4	238·0	196·4	434·4	0·0
	2·0	232·0	202·1	434·1	230·0	204·2	434·2	+0·1
	2·5	224·0	201·2	434·2	222·0	202·2	434·2	0·0
	3·0	232·0	202·2	434·2	230·0	204·2	434·2	0·0

The units are microns.

Another cylinder tested was a $\frac{1}{4}$-inch "gravity feeler" (see Table IV).

Table IV.—¼-inch "Gravity Feeler."

Millimetres from end.	B.	C.	D = B + C.	B′.	C′.	D′ = B′ + C′.	Δ = D′ − D.
2	125·0	233·3	358·3	121·0	237·2	358·2	−0·1
4	119·0	239·1	358·1	117·0	241·1	358·1	0·0
6	113·9	244·3	358·2	115·0	243·2	358·2	0·0
8	115·0	243·2	358·2	118·0	240·3	358·3	+0·1
10	122·0	235·6	357·6	124·0	233·8	357·8	+0·2
12	132·0	227·2	359·2	132·0	227·3	359·3	+0·1

The units are microns.

This cylinder should be specially perfect. The first 6 mm. are fairly uniform, but after that the variations amount to $1{\cdot}5\,\mu$. Still this and the ¾-inch gauge are much more perfect than the bar gauges with flat ends.

3. *Spheres.*—These are measured between planes. In theory this can be done by glancing contact on the same plan as for cylinders, but in this case it must be done in two directions: (*a*) up-and-down; (*b*) to-and-fro across the bed. To do this would be to use a virtual perfect plane containing the two straight lines above; no errors would occur due to an actual material plane. But the easier method is to employ end caps with plane ends and to measure between these two planes. In this case there are two preliminary operations: (*a*) measure the imperfection of the planes by methods already indicated, and, as far as possible, make the planes true; (*b*) set each plane perpendicular to the *same* screw axis and therefore parallel to one another. The latter operation is performed thus: The two translations of the table being adjusted as in § IV, para. 4, if the translations are not true straight lines the excentric cap is used, but if they are true a cap is put on the left screw point and adjusted by screws till the readings on the cap are the same at all points on a vertical and horizontal diameter. This is done by clamping a rod with contact point on the table and moving table and rod to-and-fro and up-and-down as usual. Next a cap is put on the right screw point, the rod is reversed on the table, and the right cap set in the same way.

Now since the cap surfaces are perpendicular to the same axis, they are parallel to one another even if the two screw axes be not quite parallel.

The actual measurement of the spheres is as follows: Bring up the left cap to produce electric touch with the sphere at the centre of the former. Bring up the right cap. Read both micrometer heads. Proceed in this way for various diameters of the sphere, care being taken that the table does not move during the measurements. By such means we shall always make

contact on the same points on the caps, and small defects in planeness or parallelism will not vitiate the results. The results for two 1-inch standard steel balls are shown in Table V.

The diameter of each sphere differs in various places by not more than 0·6 μ, whereas the mean value for sphere α is 1·0 μ greater than that of the other.

Table V.—Comparison of Two Standard 1-inch Balls.

Position.	α.			β.		
	A.	B.	A + B.	A.	B.	A + B.
a………………	190 ·0	252 ·7	442 ·7	191 ·0	252 ·5	443 ·5
b………………	189 ·0	253 ·5	442 ·5	191 ·0	253 ·0	444 ·0
c………………	189 ·0	253 ·7	442 ·7	191 ·0	252 ·7	443 ·7
d………………	190 ·0	252 ·4	442 ·4	191 ·0	253 ·4	443 ·4
		Mean ……	442 ·6		Mean ……	443 ·6

The units are microns.

4. *Bar Gauges with Spherical Ends.*—As a rule the radius of the spherical ends are half the length of the bar. The method of plane end caps is used as in para. 3 above.

5. *Testing the Planeness of Surfaces.*—In previous sections methods are given for gauging the accuracy of a plane metallic surface.

6. *Testing Non-Metallic Substances, e.g., Glass Plates.*—In some apparatus (*e.g.*, echelon gratings) it is important that the two surfaces of a glass plate should be parallel. The following is a method for this purpose:—*

Put an end cap on the left screw end and fasten to it by an ebonite plug (fig. 10) a steel wire having a binding screw *b* at one end and a small steel sphere at the other, the latter being just clear of the front surface of the cap. Instead of joining the circuit, as in fig. 4, bring one wire to *b* and the other to the headstock. When the screw is advanced *c* touches the glass first and is then pushed back to touch the end cap, completing the circuit through the telephone as usual.

The same fitting is put on the right screw end. The rest of the micrometry is obvious. As Table VI shows, the accuracy is slightly less than in the work on conducting bodies.

* The method of Fabry and Perot ('Ann. de Chimie,' vol. 25, p. 98, 1902) is more sensitive than the above method in a test of parallelism, but it does not give a measure of the thickness of the glass plate, as does the above.

FIG. 10.

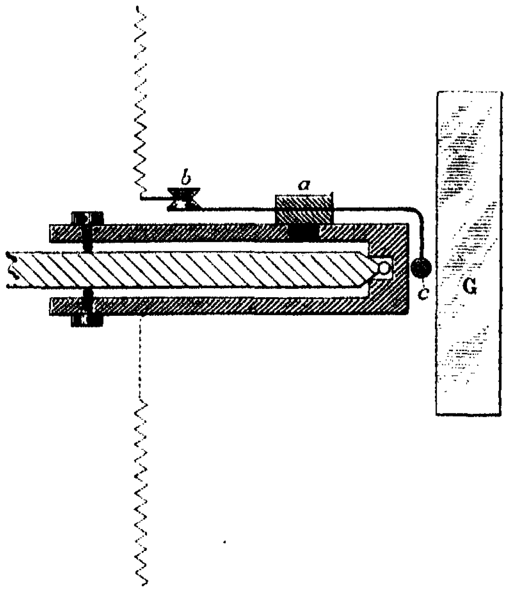


Table VI.—Optically worked Glass Plate.

Centimetres from end.	Set i.			Set ii.			$\Delta = D' - D$.
	A.	B.	D = A + B.	A′.	B′.	D′ = A′ + B′.	
0·5	329·0	100·8	429·8	329·2	100·8	430·0	+0·2
1·0	345·8	84·8	430·6	345·2	85·5	430·7	+0·1
1·5	373·1	58·8	431·9	373·5	58·4	431·9	0·0
2·0	399·1	33·2	432·3	397·5	35·6	432·1	−0·2
2·5	424·3	8·9	433·1	421·8	11·4	433·2	+0·1
3·0	463·2	−30·1	433·1	463·2	−30·4	432·8	−0·3

The units are microns.

This plate is somewhat wedge-shaped along the line of measurement selected; it is the most perfect plate tested.

§ VII.—General Remarks on the Methods.

1. The contact surfaces are polished with dry rouge and chamois leather, clamped on the table, and left there for half an hour at least to allow temperature to settle.

2. There is no spark at "break" of such length or intensity as need be

considered, the potential difference of the surfaces can be made very small, say 1/20 volt; the circuit resistance is 1000 ohms.

3. It is best to bring up right and left screws two or three times before taking readings, since the table yields slightly before the screw, but finally stops due to the small back pressure of the other screw.

4. When measuring cylinders by a glancing contact, there is a small noise as of make-and-break made when the surfaces are about 3 μ apart. This (unexplained) sound warns the observer, the true loud contact being made at the proper place with certainty.

5. The gauges used are made by the best firms. If bar gauges with flat ends cannot be considerably improved (*see* § VI, para. 1) they should be rejected unless strict point contacts are used as in this paper. But it is impossible to use point contact except with a delicate method such as that of electric touch.

6. No stress has been laid on the actual length of the gauges tested, though this could be performed with an accuracy at least as great as with usual methods. But on the comparative work on gauges shown above, stress is laid, as that work is unusually accurate. The probable error in a single reading with the electric touch is about 0·05 μ.

7. In a new machine some improvements in detail should appear:—(*a*) the beds should be stronger and the headstocks lighter, to reduce flexure of the bed; (*b*) the screws could be improved by a longer application to them of Rowland's cutting and grinding processes; (*c*) the end caps, etc., may be replaced by fittings which screw on the end of the screw-spindle, thus forming a more rigid and more easily-fastened system; (*d*) the interference bands might be from sources of cadmium, mercury, and hydrogen as used by Michelson, Pulfrich, and Tutton respectively; (*e*) a stiff spring might be put in one or both headstocks, so that, if contact be overrun, no vital part of the micrometer would be strained; (*f*) the table should be more massive. The table plays a very important part in the work, and accuracy depends more on its rigidity than one would expect without actual trial. But the general form of the table with its quick-slide movements seems quite satisfactory.

The author wishes to thank the Royal Society for the grant, which defrayed the greater part of the cost of this machine.

Bakerian Lecture.—"*Recent Advances in Seismology.*"

By John Milne, F.R.S.

(Received March 12, 1906.—Lecture delivered March 22, 1906.)

[Plate 4.]

All who are interested in the progress of science recognise that at the present time Seismology receives an attention vastly different from that accorded to it only a few years ago. The old Seismology consisted of but little more than scattered accounts of great convulsions which altered the appearance of vast tracts of country and which were frequently accompanied by appalling destruction. For the most part these accounts were little better than the narratives which, in early times, exciting the imagination of primitive communities, gave rise to myths which have left their mark in literature, art, and religion.

Not until the interval between the years 1840 and 1860 by the strenuous labours of many workers, amongst whom Perry and Mallet were conspicuous, was a general knowledge of the distribution of earthquakes in space and time reached. Mallet, in his classical work on the Neapolitan earthquake of 1859, drawn up under the auspices of the Royal Society, showed that from the ruins of a town many facts of scientific importance could be gathered. This work, together with that of a few other seismologists, gave the first indications that earthquake phenomena lent themselves to systematic investigation. But little further progress was made until 1880, when as a side issue of Japan's material development along western lines, Seismology began to grow with great rapidity into its present form and became a distinct branch of observational science.

In that year the Seismological Society of Japan was founded. The 20 volumes which it has since published furnish accounts of original research in regard to most matters connected with seismological enquiry.

The first work accomplished by the pioneers of the new science was to devise instruments which would not simply indicate that the ground had been more or less violently shaken, but which gave measurements in the form of amplitudes and periods of all the phases of ordinary earthquakes. Seismometers of types which have found acceptance throughout the world took the place of seismoscopes. Facts now replaced mere opinions. In what had been supposed to be a succession of violent back-and-forward movements it was eventually discovered that no two of these had necessarily been in the same azimuth, and that the range of these movements instead of being measurable in inches or parts of inches did not exceed 1 or 2 mm.

By the use of the new instruments it was demonstrated that the range and rapidity of movement recorded at a given station might be very different from the corresponding quantities recorded at another station only a few hundred feet distant. Experience throughout the earthquake-shaken countries of the world had long before shown that ruin was frequently very much greater in one portion of a city than in another, but the seismograph supplied actual measures of the difference in intensity of motion by which this had been brought about. Engineers and constructors were not long in recognising that earthquake destructivity could be expressed in mechanical units and, therefore, they were in a position to design structures to resist known forces.

At the Imperial University of Tokio a platform was constructed which by means of powerful machinery could be made to reproduce earthquake motion of varying intensity. On this table large models of masonry, wood and metal, designed to resist expected seismic accelerations, were tested. This table has been to the builders in Japan what a testing tank in a dockyard has been to constructors of large vessels. The ultimate result of these and other investigations has been to modify and extend the rules and formulæ of ordinary construction, and now in Japan, as opportunity presents itself, new types of structure are springing up. These have withstood violent shakings which have materially damaged ordinary types in the neighbourhood. While much has thus been done to reduce the loss of life and property, the Japanese Government, stimulated by the results of this experience, has been encouraged to extend its support to seismological investigations in general.

In 1886 the Chair of Seismology was established at the Imperial University, and since 1892 there has been in existence a Seismological Investigation Committee, which has already issued 70 quarto volumes. At the Central Meteorological Observatory in Tokio records are received from nearly 1500 observing centres. From these records we learn that in Japan between 1000 and 2000 different shocks occur annually. For each of these an approximate origin and the extent of the area of disturbance can be determined.

The first earthquake catalogue which contained facts of this description was prepared partly by assistance given by the Royal Society. It showed that Japan might be divided into at least 15 distinct seismic districts. Thus seismologists were provided with data which led to investigations that had previously been impossible. From this compilation it was also at once seen that seismic activity, and, by inference, geotectonic changes, were different in different districts and that districts marked by the greatest seismic frequency are those which afford evidence of recent secular movements of upheaval or depression. If Japan be taken as a whole, the greater number of earthquake-origins are to be found on or at the base of its steep eastern suboceanic

frontier, and only to a minor extent in the synclinal troughs of deep valleys. The greatest frequency for shocks originating beneath the ocean, Dr. Ōmori tells us, is in summer, whilst for those originating on land it is in winter. Whether this suggested seasonal distribution will be sustained by more extensive observation has yet to be determined.

In a district that has suffered from an unusually large disturbance the number of after-shocks appear to be proportional to the intensity of the initial impulse. The frequency of the movements is probably connected with the settlement of disjointed material, and its relation to time may be represented either as a formula or as a curve, from which we can roughly estimate (1) the time which will elapse before the district will reach a stable state, and (2) the approximate number of shocks which will occur during the process of settling.

From deductions based upon extensive observations we know that roughly speaking in the world some 30,000 shocks might be recorded annually. The corresponding number of megaseismic efforts is about 60. Either of these numbers may be used as an index of the present seismic vitality of our planet. Were it possible to extend this knowledge forwards or backwards through several ages we might estimate the time when hypogenic activities would fail to compensate for those the origin of which is external to our planet. The surface of our earth as it now exists has a life the length of which may yet be measurable.

The most remarkable development in modern seismology, however, is not the seismic survey of a city or even of a country, but of the whole world. This branch of enquiry is now in active progress. Since the time of the great earthquake of Lisbon in 1755 it has been known that disturbances of the magnitude of that event, although not directly recognisable as earthquakes in regions distant from the origin, have nevertheless given evidence of commotion by causing the water in lakes and ponds to oscillate. By observing and timing the movements of the bubbles of sensitive levels, astronomers have recorded unfelt pulsatory movements of the ground which they showed to be the result of seismic disturbances in far distant countries. In Japan these unfelt movements have been automatically recorded since 1884.* They were recognised to have originated at a great distance, but the centres from which they sprang were not determined. Some years later, while seeking for a gravitational influence of the moon, the late Dr. E. von Rebeur-Paschwitz found on his records abnormal movements, several of which he traced to definite but very distant seismic centres. Before this, indeed, it had been predicted that a large earthquake occurring in any one part of the world

* 'Seis. Soc. Trans.,' vol. 10, p. 6.

would produce movements which, with proper instruments, would be recorded in any other part,* but it was not until after von Rebeur's announcement that serious attention was directed to what has proved to be a line of research open to workers in all countries. Many instruments have been designed to record these unfelt breathings of our earth, but there is still much uncertainty in the interpretation of all their records.

At the present time the British Association enjoys the co-operation of 40 stations which are fairly evenly distributed over the world, and are each supplied with similar recorders. In Japan, Italy, Austria, Germany, Russia, the United States, and in other countries teleseismic movements are also observed, but the types of instruments employed are not identical. In the present experimental stage of the new investigations this diversity may be advantageous. From the records obtained from these different stations our knowledge of the earth is being increased principally in two directions. We are learning more about sudden changes that take place in the superficial covering of the globe, while new light is being thrown upon the physical constitution of its interior.

Earthquakes of the first magnitude, which disturb continental areas and frequently extend over the whole world, are in many instances if not in all, accompanied by bodily diplacements of large masses of material within the terrestrial crust. When the origin of disturbance has lain under a land surface, rough estimates have been made of the magnitude of these mass-displacements, the estimates being based upon such observed facts as the measurable length and down throw of a fault or a series of faults, the extent of compression in valleys, the alterations in relative heights, or in the lengths of lines in trigonometrical surveys. Other estimates of these quantities may be founded on the measured amounts of upheaval or of subsidence of coast-lines. Evidence bearing upon the magnitude of sudden suboceanic changes are furnished by the cable-engineer, who supplies many illustrations wherein deep-sea cables lying in parallel lines are shown to have been simultaneously broken, and where the depths previously ascertained by soundings over considerable areas are found to have been largely increased. Earthquakes which are accompanied by sea waves able to agitate an ocean like the Pacific for 24 hours, suggest that beneath the ocean there has been some fairly sudden alteration in the contour of the sea-floor.

Observations also show that large earth-waves are from time to time propagated over the whole surface of the globe. These far-reaching commotions lead to the inference that their originating impulse must have been delivered over a large region. Harboe has shown that within a meizo-

* See "Earthquakes," p. 226, 'International Scientific Series,' 1883.

seismic area blows of varying intensity have been struck in quick succession at points long distances apart. A district appears to have given way not simply along the line of one large fault, but along many minor faults. Oldham estimated that the Assam earthquake of 1897 had been accompanied by the bodily displacement of 10,000 square miles of country along a thrust plane. If we interpret the time observations made in connection with this disturbance in the light of the suggestion made by Harboe, then this relief of seismic strain originated over an area of 500,000 square miles.

Although a large block of the earth's crust may thus be fractured, our knowledge of the depth to which the effects of fracturing descend is largely one of inference. From the observations hitherto published, which are now in progress at Przibram, it would seem that a seismogram obtained at a depth of 1150 metres differs but little from one obtained on the surface. This is contrary to observations on small earthquakes which, although they may alarm the inhabitants of a town and shatter chimneys, may pass unnoticed in shallow mines.

The fact that the large earth-waves have what is practically a constant arcual velocity of approximately 3 kiloms. per second, whether the path be across continents, over ocean floors, or over districts which vary greatly in their geological structure, suggests the idea that the crust of the earth is moved as a whole, and that under the influence of its own elasticity and gravity it behaves in a manner similar to a sheet of ice upon an ocean swell. An alternative view is to assume that the wave motion is due to energy retained within the crust itself, the heterogeneity of which is superficial. Whichever be the case, we may picture a crust yielding irregularly, and possibly through its total thickness, until it gives up its energy to a medium which transmits undulatory movements with uniform velocity.

Many hypotheses have been adduced which suggest thicknesses for the superficial covering of our globe. To these as an outcome of recent seismological research we may add one more. Preceding the large waves of a teleseismic disturbance we find preliminary tremors. These are apparently propagated through the body of the globe with an average speed along paths which are assumed to be chords at about 10 kiloms. per second. This high and nearly constant rate of transmission, however, only obtains for paths which represent arcs greater than 30°. For chords which lie within a depth of 30 miles the recorded speeds do not exceed those which we should expect for waves of compression in rocky material. This, therefore, is a maximum depth at which we should look for materials having similar physical properties to those we see on the earth's surface. Beneath this limit the materials of the outer part of this planet appear

rapidly to merge into a fairly homogeneous nucleus with a high rigidity. Following closely on the heels of the preliminary tremors, but in advance of the large undulations, a second phase of motion appears, the chordal velocity of which up to distances of 120° is approximately 6 kiloms. per second. These are tentatively regarded as the outcrop of distortional waves. When these are better understood it may be expected that they also will play their part in shedding fresh light upon the physics of the earth.

I will now turn to a consideration of the regions in which these sudden accelerations of geological change are in operation. They may be grouped as follows:—

Regions which lie on the western suboceanic frontier of the American and the eastern frontier of the Asiatic continents, and regions which lie on a band passing from the West Indies through the Mediterranean to the Himalayas.

In addition to these there are two minor regions, one following the eastern suboceanic frontier of the African continent, which I have called the Malagassy region, and an Antarctic region which lies to the south-west of New Zealand.

The following table gives the number of large earthquakes or mass-displacements which have occurred in the subdivisions of these regions since 1899.

		1899.	1900.	1901.	1902.	1903.	1904.	Total.
Regions of the Pacific Ocean	1. East Indian Archipelago	11	17	13	14	11	9	75
	2. The coast of Japan	19	5	5	9	7	14	59
	3. Alaskan coast	14	11	1	1	3	0	30
	4. Central America	6	4	4	8	6	0	28
	5. West of South America	9	0	2	3	1	0	15
Western Atlantic and Eurasian regions	6. Antillian region	6	7	3	6	3	0	25
	7. Azores	13	6	3	0	2	1	25
	8. Alpine, Balkan, Caucasian, Himalayan region	4	2	8	22	22	4	62
	9. Malagassy district	9	4	4	1	3	0	21
	10. Antarctic district	Between March, 1902, and November, 1903, 75 large and small disturbances were recorded.						
	Totals	91	56	43	64	58	29	341

Many of the disturbances included in this table are known to have been followed by hundreds and even thousands of after-shocks. The most active district is at present that of the East Indies, which might well be considered as an eastern prolongation of the Himalayan region. The scene of this activity, it may be noticed, is at the junction of two lines of rock folding, which meet almost at right angles. Whether the Antillian and Central American regions should be separated is open to question. If we unite

their registers as belonging to two comparatively near and parallel earth ridges, the movements of one influencing those of the other, we have a region of hypogenic activity approximating to that of the Japan seas.

Generally it would appear that these regions of instability are to be found along the margins of continents or tablelands which rise suddenly to considerable heights above oceanic or other plains.

At the present time we may, therefore, say that megaseismic disturbances do not occur anywhere, but only in districts with similar contours. Are we dealing with primitive troughs and ridges which are simply altering their dimensions under the continued influence of secular contraction, or do these reliefs of seismic strain represent isostatic adjustments which denudation and sedimentation demand?

These and other activities may be looked to as primal causes leading up to displays of pronounced seismic activity. Their frequency, however, may be dominated by influences which at certain seasons or times cause an increase or decrease in seismic strain.

In the wide variations in position and rapidity of flow of ocean currents and in measured oscillations of sea level which appear to be seasonal in their recurrence, we see influences which may give rise to seismic frequency in districts that possess a high degree of seismic sensibility. Other causes affecting large areas and also possibly the frequency of small or after-shocks in different seismic districts have by Knott and others been sought for in the loads due to the accumulation of snow, and in the seasonal fluctuations in the direction of barometric gradients. It does not seem likely, however, that stresses due to such influences have any marked effect upon the frequency of those reliefs of seismic strain which shake the world.

The data which we possess bearing upon this question are as yet far too meagre to admit of satisfactory analysis. It is, nevertheless, interesting to note the direction in which they point. In the six years ending in 1904 we find that off the West Coast of North America 51 large earthquakes originated during the winter months (October to May) and 35 during the summer months. Off the East Coast of Asia, north of the equator, the numbers for these seasons were 49 and 43. These numbers added together show that for the North Pacific, as a whole, 100 disturbances took place in winter and 78 in summer, while in the Central Asian or Himalayan region the corresponding numbers are 25 and 27. Beneath an ocean, therefore, some indication has been obtained of seasonal seismic frequency, while on a continental surface no such frequency has yet been indicated.

If we take a chart showing the varying position of our earth's North Pole

in relation to its mean position, we see that the secular movement of the pole is by no means always uniform. Although it may at times follow a path about its mean position which is approximately circular, at other times there are comparatively sharp changes in direction of motion which may even become retrograde. If now on a chart of this description we mark the time-positions of very large earthquakes, we find that they cluster round the sharper bends of the pole path. (See Plate 4.)

In a period of nearly 13 years (1892 to 1904) I find records for at least 750 world-shaking earthquakes, which may be referred to three periods continuous with each other, and each two-tenths of a year or 73 days' duration. The first period occurs when the pole movement followed an approximately straight line or curve of large radius, the second equal period when it was undergoing deflection or following a path of short radius, and the third when the movement was similar to that of the first period. The numbers of earthquakes in each of these periods taken in the order named were 211, 307, and 232, that is to say, during the period when the change in direction of motion has been comparatively rapid, the relief of seismic strain has not only been marked, but it has been localised along the junctions of land blocks and land plains where we should expect to find that the effect of general disturbances was at a maximum. It can hardly be assumed that the frequency under consideration is directly connected with change in direction of pole movement; but it seems not unlikely that both effects may arise from the same redistribution of surface material by ocean currents and meteorological causes generally.

As we have now considered some of the more important phenomena which accompany the birth of a world-disturbing earthquake, we may next turn to its life and death. In and near an epifocal area it occasionally happens that before the vibrations which follow the first great heaving of the ground have ceased, a second violent movement may occur. In Japan this repetition has earned for itself the name of the Yuri Kaishi or "return shaking." Possibly it may be simply a second yielding within the disturbed tract, but its resemblance to its precursor suggests that it may perhaps be the resultant of some pronounced reflection. Following the initial impulse and its echo come groups of waves, separated by short intervals of time, during which movement is hardly perceptible. Although these groups as a whole grow more and more feeble, they rise and fall in their intensity. From time to time there may be repetitions of groups which have a striking similarity to each other. (See Plate 4, fig. 3.)

A world-shaking earthquake wherever its motion is pronounced gives rise to movements which may extend over three or four hours. They come to

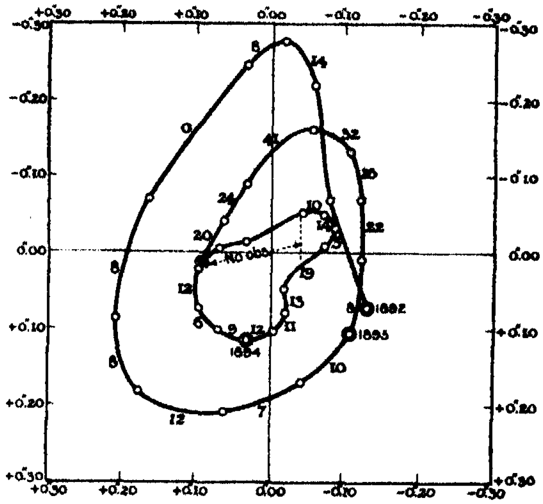


Fig. 1 shows, after Th. Albrecht, the path of the North Pole from 1892 to 1894 inclusive. Each year is divided into tenths or periods of 36·5 days. Numerals indicate the number of large earthquakes which occurred in each of these divisions, commencing with the third tenth of 1892.

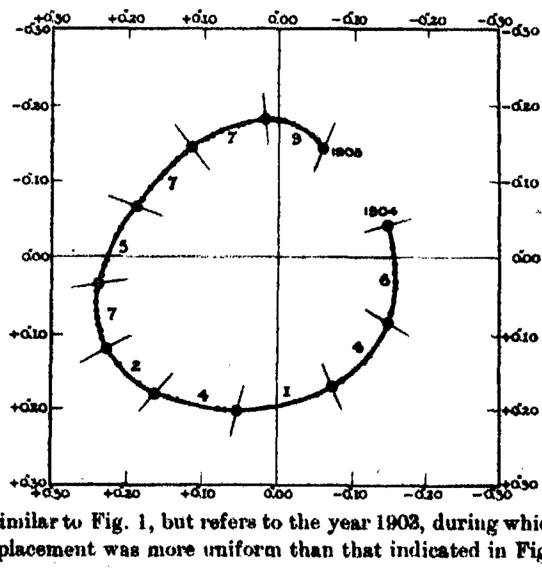


Fig. 2.—This is similar to Fig. 1, but refers to the year 1903, during which period the pole displacement was more uniform than that indicated in Fig. 1.

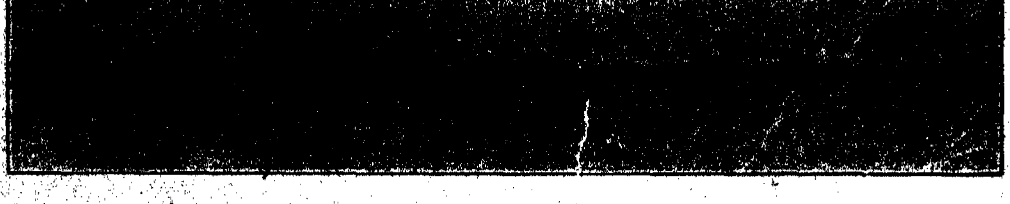


Fig. 3.—Recurrences of Wave Groups A to F in the terminal vibrations of the Colombian Earthquake of January 31, as recorded at Shide, Isle of Wight. Scale 87 mm. = 1 hour.

a close as a series of pulsations each lasting a few minutes and separated from each other by approximately equal intervals of rest. The expiring efforts of an earthquake present something more akin to musical reverberation than to intermittent and irregular settlement of disjointed material.

If instead of studying the life-history of an earthquake as recorded at a given station, we compare the seismograms it has yielded at different distances from its origin, we learn something of the manner in which its energy has been radiated and dissipated. An earthquake which in the vicinity of its origin has a duration of 60 minutes may appear at its antipodes 90 or 100 minutes later as a feeble movement with a duration of only four or five minutes. From the time this movement has taken to travel the half circumference of the globe the inference may be drawn that the surviving phase of such an earthquake is that of the large waves. The compressional and distortional precursors, together with the rhythmical succession of followers, are no longer visible on seismograms. The importance of this knowledge to those who are engaged in the analysis of earthquake-registers is apparent.

Another curious feature connected with the history of these antipodean survivors is that they may pass the quadrantal region unrecorded. What takes place may be compared to the passage of a wave down a rapidly widening estuary and to its passage up a second but similar estuary. Half-way on its journey the wave may not be perceptible, but as it converges along the latter part of its path it may again in a modified form yield indication of its existence. Other interesting investigations which have recently been made with regard to a certain class of large earthquakes refer to the peculiar form of the areas which they have disturbed. These may be described as narrow ellipses or bands which pass from an origin farthest round the world in one particular direction. The suggestion is that for this class of disturbance the primary impulse was delivered in the direction of the line of furthest propagation.

An accompaniment to earthquake radiation is seen in records of secondary earthquakes which are small and local in their character, and take place at the times when teleseismic movement has reached the district in which they are felt. A large earthquake in one district may therefore not only be regarded as the parent of many after-shocks within its own district, but it may also be related to responses in very distant places. No such relationship has yet been discovered between the larger readjustments in the earth's crust.

From the table given on p. 370 it is seen that since 1899 in the Alaskan

region seismic frequency has distinctly decreased, while in the Himalayan region it has increased. The paucity of available data, however, renders it premature to make deductions respecting possible alternation in seismic frequency in localities such as we have mentioned. But if, instead of confining our attention to a relationship between earthquakes, we consider the question of the relief of volcanic strain, many illustrations may be adduced which indicate a close connection between such activities. For example, all the known volcanic eruptions which have occurred in the Antilles from the first which took place in 1692, have been heralded or closely accompanied by large earthquakes in that region, but more frequently by like disturbances in neighbouring rock-folds, particularly that of the Cordilleras. This was notably the case in 1902. On April 19 of that year an unusually large earthquake devastated cities in Guatemala. Small local shocks were felt in the West Indies, and on April 25 it was noticed that steam was escaping from the crater of the Montagne Pelée, in Martinique. These activities continued to increase until May 8, when they terminated with terrific explosions, submarine disturbances, and the devastation of great portions of the Islands of Martinique and St. Vincent.

The last illustration of hypogene relationship between these regions occurred on January 31 of the present year. On that date a heavy earthquake originated off the mouth of the Esmeralda River, in Colombia. Sea-waves inundated the coast, islands sank, and a volcano erupted. The newspapers of February 2 announced that cables between Jamaica and Puerto Rico had been interrupted, and on later dates it was reported that severe shocks had been felt among the West Indian Islands, that six or seven submarine cables had been broken, and that Mte. Pelée and La Soufrière, in St. Vincent, were again active.

In concluding this short discourse I wish to draw attention to a class of phenomena from which the working seismologist cannot escape. At certain times horizontal pendulums may be fitfully moving continuously for hours or even days. Similar movements have often been noticed with balances and with other instruments. They are frequently referred to as microseismic disturbances. Inasmuch as they vary with varying meteorological conditions, and may be different in neighbouring rooms, I am inclined to think that it would be more accurate to describe these unwelcome visitors, with which not only seismologists, but also astronomers and others have to contend, as air tremors. When, however, these irregular movements are replaced by movements which have definite periods very different from those of the recording instrument itself, and are at the same time regular in amplitude, it seems possible that they may be connected with actual

pulsatory motion of the surface of the ground. In addition to tremors and pulsations, the records on the films from seismographs show that nearly at all times a slow change of level is taking place. For years a pier may be undergoing a tilt in one direction. Besides this general movement the instruments reveal the existence of waves that indicate a difference in the direction of movement in different seasons. Superimposed upon these again we find records of changes of level which may be associated with variations in the difference in loads on two sides of an observing station. When a horizontal pendulum swings towards the area of greatest atmospheric pressure it apparently indicates a change directly or indirectly connected with barometric loading. The quantity of water in wells and that flowing in drains and from springs has been observed to vary with fluctuations in atmospheric pressure. Where this takes place, sub-surface operations are revealed which may be sufficient to give rise to changes in surface level. When a squad of 76 men marched to within 16 or 20 feet of the Oxford University Observatory it was found that a horizontal pendulum inside the building measured a deflection in the direction of the advancing load. The observation that a surface dips in the direction of a load it carries may however be unexpectedly modified. The concrete floor of a cellar on the strand at Ryde has with the rise of the tide in the Solent been observed to tilt towards the land, whereas the anticipated direction of change in level was in the contrary direction. In this instance the rising water in all probability masked its own gravitational effect by backing up sub-surface drainage, with the result that the foreshore was floated or lifted upwards. Very marked changes of level take place at certain stations during wet weather. In the Isle of Wight, at Shide, which is situated on the side of a valley cut through an anticline of chalk, when heavy rain occurs, levels and horizontal pendulums indicate a tilting towards the bed of the valley. An instrument on the opposite side of the valley behaves in a corresponding manner. In other words, if these observed movements can be regarded as extending to the bed of the valley, it may be said that with rain the steepness of each of its sides is increased. During fine weather the direction of movement is reversed. A more regular movement is, however, found in a tilting known as the diurnal wave. With the same assumption as to the extent of corresponding motion we find, but only during fine weather, that the direction of movement of the sides of the same valley during the night corresponds to that observed during wet weather. During the day it is the same as that which takes place during fine weather. For convenience we may regard the valley as opening and closing. Similar observations have been made on the two sides of a valley which has been cut through alluvium in Tokio.

This diurnal movement is only marked on days which are bright and sunny. On dull, cloudy, or wet days it is small or not recordable. In a chamber 13 feet beneath the surface, excavated in soft ground where changes in temperature are very small, I have found the diurnal movement to be quite as marked as at neighbouring installations on the surface, where temperature changes were comparatively large. I have not observed it in excavations made in rock at depths of 50 and 100 feet. At Bidston, however, in the New Red Sandstone, at the depth of 19 feet, changes of 0″·1 and 0″·2 are from time to time recorded. On flat, open country the variation is small at all times. An influence which probably plays an important part in the production of these movements may be sought for in the differential loading and unloading of neighbouring areas by solar influences. During wet weather, in virtue of sub-surface percolation and lateral drainage generally, the sides and bottom of a valley where water-level is raised, carry a greater load than the bounding ridges. Under these conditions the bottom of a valley may sag and its sides close inwards. During fine weather, in virtue of evaporation and drainage, a movement in the opposite direction may be established. The fine-weather diurnal movement corresponding to the opening of a valley may find a partial explanation in the removal of load by evaporation, but more particularly by plant-transpiration. These activities are more pronounced during the day than at night, and they tend to reduce sub-surface percolation and drainage towards the bed of a valley. The comparatively small retrograde nocturnal movement may be partly attributed to an increase of valley load at night, at which time transpiration and evaporation are replaced by surface and sub-surface condensation. Transpiration and evaporation being at a minimum at night, it may be assumed that lateral percolation and surface drainage towards the bed of a valley are increased, and possibly as a consequence of this action, the volume of water in certain wells and that flowing in certain streams and drains has been found to be greater at night than during the day.

Another activity which may result in a nocturnal increase in the sub-surface flow of water is the expansion of the air in soil by the slowly descending heat of the previous day, this expansion forcing soil-water into passages of easiest escape.

The explanation offered for the phenomena under consideration may be found wanting; but the facts remain, that round the face of the globe diurnal superficial distortions can be observed which vary in magnitude and direction, and that rainfall is accompanied by measurable changes in the slopes of certain valleys. These surely are facts that deserve recognition.

Ionic Size in Relation to the Physical Properties of Aqueous Solutions.

By W. R. BOUSFIELD, M.A., K.C.

(Communicated by Professor Larmor, Sec. R.S. Received January 5,—Read February 8, 1906.)

(Abstract.)

The electrical conductivity of a solution depends upon the rates of transference of the ionised portions of the solute in opposite directions under the influence of the applied electromotive force. These rates of transference under a given potential gradient are conditioned by the viscosity of the medium, and the sizes and possibly the shapes of the ions. Increase of viscosity of the solution and increase in the sizes of the migrating ions both tend to diminish the rates of transference of the ions, and thus to lower the conductivity. If the ion enters into combination with one or more molecules of water, its size is necessarily increased, and the motion of the water-logged ion becomes more sluggish as the amount of water in combination increases. To separate the elements which determine the conductivity of an electrolytic solution, and to analyse the joint effect of variations in ionisation, viscosity, and water combination is a matter of great difficulty, but of much importance to the theory of solution.

In a former paper (Bousfield on "Ionic Sizes in Relation to the Conductivity of Electrolytes")* was proposed a method for effecting such an analysis based upon the expression evaluated by Stokes for the terminal velocity of a small sphere moving in a viscous medium. A consideration of the influence of the water in combination with the ion upon its mobility was used to obtain a correction of the coefficient of ionisation which made Van't Hoff's law (in a slightly modified form) an *accurate* expression of the relation between ionisation and dilution, down to twice decinormal solutions of KCl. This method of procedure gave for the radius of the hydrated ion an expression of the form

$$r = r_\infty(1+\mathrm{B}h^{-\frac{1}{2}})^{-1},$$

which indicated that the average radius of the ion steadily increased with dilution, owing to increasing hydration of the ion, up to "infinite dilution."

* Communicated to the Royal Society, February 10, 1905. Revised and published in the 'Zeit. für Phys. Chem.,' vol. 53, p. 257, 1905.

Whether the resulting expression for r did in fact represent the average radius of the ion was tested by a consideration of the density law which would thence result. The volumes of the ions would be proportional to r^3, and it was found that a rational density formula could be constructed upon this basis which accurately corresponded with the observed densities of the solutions.

In the present paper, the necessary experimental determinations for applying the method to solutions of NaCl are given, together with some other determinations for collateral purposes, and the hypothesis is further tested by reference to other physical properties of solutions. These results are independently of some value, apart from the hypothesis by which they are reached, and it has been thought well to designate the function r, which, according to our view, expresses the average radius of the ion, by the term "radion." Whilst this term connotes our hypothesis, it may, if necessary, be merely considered as expressing a certain function of the dilution. But it has a further convenience as it enables us readily to extend the conception to denote the average molecular radius of any group of ions or molecules, or even of the whole of the ions and molecules both of solvent and solute in any given solution. The utility of this extended conception will appear more clearly in the section relating to a consideration of ionic size with reference to the viscosity of a solution.

The volume of the ion, according to our hypothesis, is proportional to the cube of the radion, and the volume of a pair of ions to the sum of the cubes. These cubes and their sums we refer to as "ionic volumes." But, except where the context indicates the contrary, the term "radion" may be taken as merely denoting the function

$$r = r_\infty(1+\mathrm{B}h^{-\frac{1}{3}})^{-1},$$

and the term "ionic volume" as denoting the cube of the radion or sum of the cubes of the radions, apart from the hypothesis as to size.

As in our former paper with reference to KCl, it is shown that the "solution volume" of NaCl solutions is a linear function of the ionic volumes. Hence the densities of KCl and NaCl solutions can both be accurately expressed by the same formula, as simple functions of the radions.

A theoretical consideration of the relation of the Hittorf migration numbers to the sizes of the ions is given, and it is shown that our hypothesis as to the influence of ionic sizes upon rates of transference, would theoretically lead to the result that the reciprocals of the Hittorf migration numbers should be expressible as a linear function of the ratio of the radions. This turns out, in fact, to be the case, and we incidentally

arrive at a useful method of extrapolation to determine the value of the Hittorf number for an electrolyte at "infinite" dilution.

These considerations enable us to determine the coefficients B for the separate ions in the expressions for the radions. These coefficients B we refer to as the "hydration numbers," the relation between the hydration numbers and the migration numbers at infinite dilution being of the form—

$$B = B_1N_1 + B_2N_2.$$

Turning now to a consideration of the viscosities of the KCl and NaCl solutions, it is shown that the viscosity of dilute solutions can be represented approximately as a linear function of the radions, as can also the viscosity of mixtures of normal KCl and NaCl solutions.

Passing to a consideration of the general relation of viscosity to ionic size, the extended conception of the radion is introduced, and an approximate value is given to the radion of water, which expresses the average radius of the water molecules reckoned upon the same scale as the radions of the solute. Using this value of the water radion in conjunction with the values of the radions of the solute determined from the conductivities, it is shown that the viscosity of the solutions can be expressed with a fair approach to accuracy by the expression

$$\eta = C\Sigma\beta r,$$

where r stands for the radion of a given species of molecules and β for the fraction of the total volume occupied by such species. Since $\Sigma\beta r$ is, upon the extended conception of the radion, the average molecular radius of the whole solution, we may express this result by saying that the viscosity of a solution is proportional to its radion.

In order to correlate ionic sizes with osmotic pressure a prolonged attempt was made to measure the vapour pressure of dilute KCl solutions at 18° C. A large barometer tube was used, closed by a small tap at the bottom, so that minute differences of level could be determined by removing and weighing the mercury cistern. A similar arrangement was used to determine simultaneously the variations of atmospheric pressure during each observation. It was found, however, that the variations of atmospheric pressure were often larger than the differences of vapour pressure to be measured, and no sufficiently accurate results could be obtained. Recourse was therefore had to the freezing-point determinations of Jahn with KCl and NaCl solutions. The variations of ionic size with temperature are probably serious and at present* unknown, and hence a consideration of ionic sizes

* In my former paper I attempted to calculate the variation of ionic size with temperature by reference to the conductivity temperature coefficients of the ions at infinite

at 18° C. in relation to osmotic data at another temperature might lead to error. But it seemed probable that the ionic sizes at different dilutions might have the same *relative* values at 18° C. and 0° C., and in the absence of other data it was decided to use these.

Defining the "effective molecular freezing-point depression" as the ordinary so-called molecular freezing-point depression divided by $(1 + \alpha)$, where α is the ionisation, and denoting it by the letter D, it was found that D was a linear function of the ionic volume, and that it could be expressed both for KCl and NaCl as

$$D = 1{\cdot}86 + C(I_{\infty} - I_V),$$

where I_V stands for the ionic volume at the given dilution and I_{∞} for the ionic volume at infinite dilution, the value of the constant C being nearly the same for both substances.

In addition to the confirmation thus afforded to our view as to the fundamental importance of the radion in the theory of solutions, we are further led to a useful formula for obtaining by extrapolation the value of the molecular freezing-point depression at "infinite" dilution. For this purpose we are able to dispense with our hypothesis, and obtain from it a new result quite independent of it—one of the recognised tests of the validity of a hypothesis, though not a conclusive one.

We saw that the solution volume was a linear function of the ionic volume, and we have now the effective molecular freezing-point depression also as a linear function of the ionic volume. Hence the effective molecular freezing-point depression should be a linear function of the solution volume, and in this case the reference to ionic sizes which correlated the two sets of phenomena can be dispensed with. In order to test this matter, measurements of the densities of KCl and NaCl solutions at 0° C. were made. This is a little above the freezing point of dilute solutions, but it was considered to be near enough to make the desired comparison. Density measurements were made upon solutions of strengths of 1/2, 1/4, 1/8, 1/16, and 1/32 normal, and empirical formulæ (based on the lines of the rational formulæ for 18° C.) were constructed to obtain the solution volumes at the concentrations at which Jahn's freezing-point determinations were made.

dilution which were given by Kohlrausch. I have, however, since come to the conclusion that these results are unreliable. Kohlrausch's values were largely based on determinations of the conductivities of 1/1000 normal solutions. Dissociation being incomplete in such solutions, any variation of conductivity due to change of ionisation with temperature would be included in his temperature coefficients and might entirely vitiate the deductions which I drew. This portion of my former paper must therefore be withdrawn, and I propose to pursue the matter further experimentally

The result was that the effective molecular freezing-point depression, both for KCl and NaCl solutions, could be expressed by one formula with the same constants for both substances, viz. :—

$$D = 1{\cdot}86 + 3{\cdot}3\delta V_s,$$

where δV_s is the change in the solution volume for different dilutions. This formula would also include a non-electrolyte such as sugar, in which δ is almost zero.

In this sketch of the course of the present paper, we have passed over some matters arising incidentally which may call for mention. But it must be observed that the main purpose kept in view throughout, is to show the interpenetration of the theory of ionic sizes with the theories of the various phenomena of solutions, and to test the theory as far as possible in its relation to such phenomena. In this process various side avenues have been opened up, which we have forborne to follow if they carried us too far from the main track.

One incidental matter of importance is the correction of the coefficient of ionisation which is afforded by the theory, according to which the true value of the coefficient of ionisation is

$$\alpha = \frac{\lambda}{\Lambda} \cdot \frac{\eta}{1 + Bh^{-\frac{1}{3}}}.$$

The result of this correction is to make the values of α for KCl and NaCl *identical* for equimolecular solutions, down to twice decinormal concentration. An accurate empirical formula for α was proposed by Kohlrausch* of the form

$$\frac{1-\alpha}{\alpha^p} = Cm^{\frac{1}{p}}.$$

The values of the constants given by Kohlrausch were :—

	p.	C.
KCl	3·280	0·7190
NaCl	2·649	0·7707

Thus two sets of different constants are required to give the values of α for the two substances at the same concentration. The values of the constant p for the two substances differ by about 20 per cent. The resulting values of α at twice decinormal concentration differ by about 3 per cent. Our values of α, which are corrected for size of ions and viscosity of solution, being identical, are given by one formula with the same constant for KCl and NaCl, viz. :—

$$\frac{h}{\alpha} \Big/ \left(\frac{h}{1-\alpha}\right)^{\frac{2}{3}} = 3{\cdot}197.$$

* 'Sitz. der K. Preus. Akad. der Wiss. zu Berlin,' vol. 44, p. 1002, 1900.

This matter was further tested by determining the densities of mixtures of normal solutions of KCl and NaCl, and it was found that the observed densities were correctly given by the "law of mixtures" within very narrow limits.

The conductivities of these mixtures were also determined and a mixture law for the conductivities based upon the consideration of viscosities and ionic sizes was formulated. The agreement thus obtained between observed and calculated values also tends to show that the hypothesis upon which the calculations were made is correct.

The increase in the ionic volumes which takes place with increasing dilution must be nearly proportional to the increasing volume of water combined with the ion. The development of this matter quantitatively so as to determine the number of molecules of water in combination with the ion under different circumstances is a matter of great importance, but it cannot be dealt with in this paper. The effect of water combination as a sufficient and possibly the only cause of ionisation, is dealt with, to some extent, in the former paper,* and has also recently been considered by Lowry (An Application to Electrolytes of the Hydrate Theory of Solution).†

Concluding Observations.

Let us now briefly review the main course of the argument, as developed in this and the former paper, in relation to our fundamental hypothesis.

As our starting point we took the Van't Hoff dilution law, which we may express by saying that if D represents the concentration of the dissociated portion of a solute and U the concentration of the undissociated portion, reckoned by means of the ordinary value of α, there exists a linear relation between log D and log U, leading to a relation of the form $K = D/U^n$, where n is for various electrolytes nearly but not quite two-thirds. (For KCl $n = 2{\cdot}2/3{\cdot}2$ about.)

This suggested that if we could find a suitable correction for α, which is usually taken as λ/Λ, the Van't Hoff dilution law would turn out to be an *exact* relation for dilute solutions of binary electrolytes such as KCl.

The materials for such a correction were sought in the known fact that the viscosity of the solution produced aberrations in the mobilities of the ions, but viscosity differences alone were inadequate to give an account of such aberrations.

Kohlrausch's observations on the temperature coefficients of the ions had

* See the revised version published in the 'Zeitschrift f. Phys. Chem.,' *loc. cit.*

† 'Trans. Farad. Socy.,' vol. 1, p. 197, 1905.

already led him to the general view that the ions must be considered to be water-coated.* This water combination necessarily altered the sizes of the ions, and it was considered that the joint effect of changes of viscosity and changes of size might adequately account for changes in the mobility.

To reckon these effects quantitatively, Stokes' theorem as to the motion of a small sphere in a viscous medium was available, and though the actual motion of the ions through an electrolyte under the influence of a potential gradient is probably extremely erratic, it was thought that, nevertheless, the effect of size and viscosity upon the average rectilinear drift under the influence of the electromotive force might be amenable to exact treatment, just as the average rectilinear drift itself can be accurately calculated.

Assuming, then, that the aberrations of the mobilities which made the Van't Hoff law inexact were due to such causes and could be dealt with in this way, corrections were applied to the mobilities and corrections for α were calculated, the nature of which was determined by the Stokes theorem, and the amount of which was determined by the Van't Hoff law (expressed in terms of the hydration h instead of the volume V of the solution).

The result of this process was to give us the expression

$$r = r_{\infty}(1+\mathrm{B}h^{-\frac{1}{3}})^{-1}$$

for the relation between the radius of an ion and the dilution of the solution.

Up to this point, if one may compare small things with great, the process followed is similar to that of the astronomer who sought to locate the position of a new planet by considering the irregularities produced in the movements of the old ones. In that case the result could be tested by turning a telescope to the spot indicated by the calculations.

In the present case the result could only be tested by considering how far the hypothesis as to the changing sizes of the ions owing to the changes in the amount of water combination could be rationally related to the various physical phenomena of solutions, and how the quantitative results were functionally related to existing data. In the case of each set of phenomena it was necessary to consider *à priori* how the changing size of the ions would be likely to affect it.

À priori it seemed probable that the Hittorf transference numbers, and the viscosities of the solutions themselves, would depend merely upon the linear dimensions of the ions, whilst the densities of the solutions and the variations of effective molecular freezing-point depression would depend upon

* 'Roy. Soc. Proc.,' vol. 71, 1903, p. 338.

the amounts of combined water, and therefore upon the cubes of the ionic dimensions.

These *à priori* considerations have, in fact, turned out to be justified, not merely qualitatively, but with considerable numerical accuracy, having regard to the difficulty of some of the approximations involved. We are able to express the Hittorf numbers, the densities, and the effective molecular freezing-point depressions within the limits of experimental error, as simple functions of the radions, and to express the viscosities with a fair approach to accuracy, not merely as a function of the radions of the solute, but also upon the extended conception of the radion, as being simply proportional to the radion, or average molecular radius, of the whole solution. Our hypothesis has also enabled us to predict two new relations which are independent of the hypothesis, viz., the fact that the Hittorf migration numbers are a linear function of $(B+h^{-\frac{1}{3}})^{-1}$ and the fact that the effective molecular freezing-point depression is a linear function of the solution volume.

It is submitted that the above considerations justify the working hypothesis that the function which we have named the radion, derived as above described, may in fact be taken to be a measure of the actual sizes of the ions. In any case the radion turns out to be of fundamental importance in correlating the various phenomena of solution.

A Discussion of Atmospheric Electric Potential Results at Kew, from selected Days during the Seven Years 1898 *to* 1904.

By C. Chree, Sc.D., LL.D., F.R.S.

(Received February 21,—Read March 15, 1906.)

(From the National Physical Laboratory.)

(Abstract.)

The Paper contains an analysis of the atmospheric electricity results at Kew from selected days—usually 10 a month—during the seven years 1898 to 1904.

The days were chosen as representative of fine weather conditions, all days being excluded when rain fell or negative potential was recorded. By means of regular observations with a portable electrometer, the curve readings from the Kelvin water-dropping electrograph were converted so as to give the potential gradient in the open, in volts per meter. All the data: mean monthly values, diurnal inequalities, etc., are given in absolute measure (volts per meter). This is believed to be the first occasion on which this has been done. The diurnal inequalities for individual months and for the whole year are represented by curves. These are mostly exceedingly smooth, thus showing that a sufficient number of years' data have been included to give satisfactory results. The curves all show two distinct daily maxima and minima. The minima occur in all the months near 4 A.M. and 2 P.M. The times of the maxima are more variable, the day interval between the forenoon and the evening maximum being longer in summer than in winter.

The month showing the highest mean potential gradient is December, but the amplitude of the diurnal inequality is greatest in February. Whilst the amplitude of diurnal inequality, when considered absolutely, is greatest in the mid-winter months, the ratio in which it stands to the mean daily value is least at this season.

The diurnal inequalities for the several months are analysed in Fourier series, with 24-, 12-, 8-, and 6-hour terms. The 12-hour term is, in general, the most important, especially in summer; the changes in its amplitude and phase angle throughout the year are comparatively small. The 24-hour term is much more variable. It is much larger in the winter than in the summer months, and its phase angle varies greatly throughout the year.

The diurnal range, the 24 hourly differences from the mean for the day, and the amplitudes of the 24- and 12-hour waves, have their annual variation

expressed in Fourier series, with annual and semi-annual terms. In all cases the annual term proves to be the more important.

Attention is given not merely to the regular diurnal changes, based on mean results from a number of days, but also to the phenomena exhibited by the individual days themselves. It is found that the difference between the highest and lowest hourly values is, on the average, two and a-half times the amplitude of the regular diurnal inequality, and is, in fact, fully larger than the mean value for the day of the potential gradient.

The difference between the values of the potential gradient at successive midnights of the selected days, when taken irrespective of sign, averages about 43 per cent. of the mean daily value. When taken algebraically, there seems a slight tendency in the potential to rise during the selected days in December and January, but, taking the year as a whole, the mean non-cyclic element is exceedingly small.

The possible influence of various meteorological elements is considered from several standpoints. The influence of temperature is found to be much the most marked, there being a clear association of high mean potential and large diurnal range of potential with low temperature in every month of the year, except the hottest (July). In the winter months there is also an association of high potential with low wind velocity and high barometric pressure, but the association in these cases is much less clear.

Some of the data are compared with older data for Kew, obtained by Everett. In some respects there is fair agreement, but conspicuous differences exist. The results are also compared with other data given in a recent important memoir by Mr. A. B. Chauveau, for Kew and Greenwich, and for a number of stations in Italy and France, especially the Bureau Central Météorologique and the Eiffel Tower in Paris.

An Appendix makes a minute comparison of the diurnal inequalities of potential and of barometric pressure at Kew. A somewhat striking resemblance between the diurnal changes in these elements was pointed out by Everett in 1866, which possesses increased interest of late years, owing to Elster and Geitel's discovery that air extracted from the soil is usually markedly ionised, and their consequent suggestion that the variations of barometric pressure may influence the potential gradient by facilitating or retarding the escape of this ionised air into the atmosphere.

Everett's original comparison was between potential data from Kew and barometric data for Halle, both for the mean diurnal inequality from the whole year. Both elements are, in reality, considerably dependent on local conditions, and it thus seemed important to employ data for the same place. Mean diurnal inequalities were thus got out for each month of the year for

the barometric pressure at Kew, making use of the data published in the "Hourly Means" of the Meteorological Office for an 11-year period, 1890 to 1900. This enabled a really critical comparison to be carried out with the potential gradient. The result shows decisively that the similarity between the diurnal inequalities of the two elements is confined to the 12-hour terms; the 24-hour terms present, in fact, diametrically opposed phenomena in the two cases. The afternoon minimum and evening maximum of potential are in every month notably in advance of those of barometric pressure, and the 12-hour potential wave is about an hour in advance of that of barometric pressure throughout the whole year. Thus, if any relationship of cause and effect exists between the *regular* diurnal changes in the two elements, the pressure change would seem to be the effect, the potential change the cause.

Explosions of Coal-Gas and Air.

By Bertram Hopkinson, M.A., M.I.C.E., Professor of Mechanism and Applied Mechanics in the University of Cambridge.

(Communicated by Professor Ewing, F.R.S. Received January 19,—Read February 8, 1906.)

Explosions of Coal-Gas and Air.

The experiments here described consist in an investigation into the propagation of flame through a mixture of coal-gas and air contained in a closed vessel and ignited at one point by an electric spark. A continuous record is taken of the variation of resistance of fine platinum wires immersed in the gas, at different points; and at the same time and on the same revolving drum the pressure is recorded. The arrival of flame at any wire is marked by a sharp rise in its resistance. Thus the progress of the flame can be traced. Moreover, the rate of rise of temperature of the wire after the flame has reached it is (after certain corrections have been applied) a measure of the velocity with which the gases round about it combine. In this manner it has been possible to settle in the case of certain mixtures, at any rate, the question of "after-burning," which has long been a matter of controversy in the theory of the gas-engine, and to determine approximately the specific heat of the mixture of CO_2, H_2O, and inert gases which are the products of the combustion. Incidentally it has been necessary to find what relation the temperature of a fine platinum wire immersed in the

heated gas bears to that of the gas. Burstall, who has measured the temperature in a gas-engine cylinder by means of platinum wires, did not fully investigate this point, and his results are, in consequence, open to doubt.*

Before proceeding to a detailed account of the apparatus and records obtained it will be convenient to state shortly the principal conclusions reached. The experiments were all made on mixtures of air and Cambridge coal-gas having an average "higher" calorific value of 680 British Thermal Units per cubic foot at 0° C. and 760 mm. The composition of the gas is given in an appendix. The mixture was fired at atmospheric pressure in a vessel of dumpy cylindrical form and of a capacity of 6·2 cubic feet, which is shown in section in fig. 1. The combustion was started by an electric spark at the centre of the vessel.

Explosion of Rich Mixture.

With nine volumes of air to one of gas the maximum pressure varied from 76 to 82 lbs. per square inch above atmosphere; and was reached about 1/4 of a second after firing; on fig. 2 is shown a facsimile of a pressure diagram (curve A). It was found with a mixture of this composition (air/gas = 9) that:—

(1) The flame spreads from the spark with a velocity which varies somewhat in an apparently accidental manner, but which is roughly 150 cm. per second. The spread of the flame is of a rather irregular character, and differs slightly in different directions; Mallard and Le Chatelier found for a mixture containing 17 per cent. coal-gas, a velocity of flame propagation of 125 cm. per second along a tube of 2 or 3 cm. diameter.†

(2) The flame reaches the walls when the pressure is of the order of 15 to 20 lbs. per square inch above atmosphere. At this point, however, only a small portion of the walls is in contact with the flame, namely, that nearest the spark, and most of the gas is still unignited. As the flame spreads a greater and greater area of the walls comes into contact

* 'Proc. Inst. Mech. Engrs.,' 1901. The only attempt to apply the methods of platinum thermometry to gaseous explosions of which I am aware, is this one by Burstall. He could not use very fine wire because it melted, and it seems probable that on this account his temperatures are a good deal wrong, even in the latter part of the explosion, and his results give no information as to the initial stages of the burning and throw no light upon the question of the velocity of combination. He used a rotating contact maker, which made contact at definite epochs in the cycle, but did not give a continuous record of temperature in any one explosion.

† "Recherches sur la combustion des Melanges Gazeux Explosifs."

with it, until the flame completely fills the vessel and is losing heat to every part of it. At this point the pressure is still a little short of the maximum, being about 70 lbs. per square inch when the maximum pressure reached is 82 lbs. Maximum pressure is attained in less than 1/30 of a second after the flame completely fills the vessel.

(3) At the centre of the vessel the temperature of the gas rises very rapidly, after ignition, to about 1225° C. It reaches that figure, within 50°, in a time which is certainly less than 1/20 of a second, and probably less than 1/50 of a second; in other words, the combustion is complete within about 4 per cent. in that time. The temperature remains nearly steady during the earlier part of the spread of the flame, the pressure during this time remaining very nearly constant. The combustion at the centre takes place very nearly at constant pressure, and is complete within 5 per cent. before the pressure has risen more than a couple of pounds above atmosphere. From this result, if it be assumed that no heat is lost until the flame reaches the walls of the vessel, it follows that the capacity for heat at constant pressure, reckoned between 50° and 1200° C. of the products of the explosion, is about 1·5 times that of the same volume of air. There is no doubt that the flame radiates some of the heat of combustion, but it is improbable that the loss from this cause exceeds 15 per cent. If that percentage be assumed then the capacity for heat is 1·3 times that of air.

(4) In the adiabatic compression of the gas in the centre, which takes place in the later stages of the explosion, the temperature rises to a point which is considerably above the melting point of platinum — probably about 1900° C. If the ultimate temperature, corresponding to a compression of 6·5 atmospheres absolute, is 1900° C., then the average value of γ (ratio of specific heats) for these gases is 1·25 between 1200° and 1900° C.

(5) A platinum thermometer, placed about 1 cm. within the walls at the furthest point from the spark, reaches its maximum temperature, which varies accidentally, but is between 1100° and 1300° C., within 1/30 of a second after the attainment of maximum pressure. There is here but little adiabatic compression or rise of temperature after ignition. The gas at this point has been compressed to about five atmospheres before ignition, and the temperature due to this compression is about 200° C. Experiments by Petavel,* in which coal-gas and air were exploded after compression to about 77 atmospheres, show that the rise of temperature on explosion is nearly independent of the pressure. If therefore there were no loss of heat, the temperature at this point would rise to about 1400° C. At the same instant the temperature at the centre is 1900° C. Consequently, even if the

* 'Phil. Mag.,' May, 1902.

explosion were to take place in a vessel impervious to heat, there would be a difference of 500° C. between the maximum and minimum temperatures in the vessel. Similar differences must exist whenever a mixture is fired at discrete points; the temperature at the firing points will always be raised much above the mean by adiabatic compression after ignition.

(6) It seems certain from these experiments that in a mixture of this strength combustion is for practical purposes everywhere complete at the time of maximum pressure, and that subsequent to that time the gas is a mixture of CO_2, H_2O, and inert gases in chemical equilibrium.

(7) At the moment of maximum pressure the distribution of temperature is roughly as follows:—

Mean (inferred from pressure)	1600
(*a*) Centre near spark	1900
(*b*) 10 cm. within the wall (C, fig. 1)	1700
(*c*) 1 cm. from wall at end (D, fig. 1)	1100 to 1300
(*d*) 1 cm. within the wall at side	850

At points *a*, *b*, and *c*, the gases can have lost but little heat at this time, and the differences of temperature are almost wholly due to the different treatment of the gas at different places. At (*a*) it has been burnt nearly at atmospheric pressure, and compressed after burning to about six and a-half atmospheres absolute, while at (*c*) it has been first compressed to about six atmospheres as in a gas-engine, and then ignited without any subsequent compression. At the point (*d*) much heat has been lost, since this is the first point on the wall reached by the flame; the gas here is ignited when the pressure is about two atmospheres, its temperature rises instantly to 1300°, and at once begins to fall.

(8) Up to the time of maximum pressure convection currents in the gas have had but little effect upon the distribution of temperature, which is mainly determined by the treatment accorded to the gas at different points. After combustion is complete, however, the motion of the gas set up by the explosion or by convection currents rapidly obliterates these initial differences. Half a second after maximum pressure the distribution of temperature is as follows:—The mean temperature of the whole of the gas (calculated from the pressure) is about 1100° C. The mean temperature, exclusive of a layer 1 cm. thick in contact with the walls (shown by the resistance of a long wire stretched from the centre to D, fig. 1), is about 1160° C. The temperature of the hot core is fairly uniform, though it varies in an accidental manner about the mean value; thus, at the centre, in different explosions, I have found temperatures ranging from 1100° to

1200° C. at this time. The mass of gas during cooling may therefore be described as a hot core in which the temperature is approximately uniform (though it varies accidentally as the result of currents) surrounded by a thin layer wherein the temperature falls to the temperature of the walls. I find, by calculation, that if such layer were ½ cm. thick, and if the fall of temperature were uniform, the mean temperature inferred from the pressure would fall short of that of the hot core by about the observed amount, viz., 60° C.

Explosion of Weak Mixtures.

The explosion of a weak mixture containing 12 volumes of air to 1 of gas differs markedly from that of a 9 to 1 mixture. In the latter case there is no time for the buoyancy of the light burnt gas to materially affect the propagation of the flame, though it doubtless causes the apparent velocity of propagation to be slightly greater in an upward direction than downward. Convection currents have no material influence on the phenomena until after the attainment of maximum pressure. But in the weaker mixture their effect is important from the outset. The small portion of the gas first ignited instantly rises with increasing velocity. At the same time it grows, by the ignition of that surrounding it, but at a rate which soon becomes considerably less than the velocity with which it is rising. In spite of the very slow propagation of flame from point to point, however, the combination of the gases, once initiated, is rapid. Thus the temperature of a wire placed close to the spark rises within 0·07 second to about 1000° C., and then remains nearly stationary for some time. A few centimetres below the spark the temperature will rise rapidly and then fall; the flame reaches the wire, and is then carried upward and away from it, the wire being cooled by the current of cold, unburnt gas which follows in the wake of the ascending flame. About 1 second after ignition, and while the pressure is still less than 10 lbs. above atmosphere, the upper half of the vessel is filled with burnt gas which is in contact with, and losing heat to, the upper half of the walls. In the lower parts of the vessel the gas is still unburnt. The last portions of gas to be ignited are those immediately under the spark, and from 10 to 20 cm. away from it. A wire placed at this point shows a gradual rise of temperature, due to the adiabatic compression, followed by a sudden rise, due to ignition, slightly before the time of maximum pressure. In general, a platinum wire placed anywhere within the vessel shows, at some time *before* maximum pressure, a sharp rise in temperature lasting about 1/10 of a second, after which the temperature is steady for a time, and then falls slowly. There are fluctuations of temperature both up and down, but these are plainly

accidental effects of convection currents, and are due neither to the ignition of portions of unburnt gas nor to the slow combination of gas already ignited.

My experiments seem to show that in the weakest inflammable mixtures and in strong mixtures alike, the combustion once initiated at any point, is completed almost instantaneously. Moreover, the complete inflammation of the gas is, even in the weakest mixture, nearly simultaneous with the attainment of maximum pressure. One-tenth of a second before maximum pressure there are undoubtedly places to which the flame has not spread, and it is extremely improbable that there are any such places 1/10 of a second after. It is safe to assume in dealing with a 12/1 mixture that 1/5 of a second after maximum pressure (when the loss of pressure by cooling is still less than 5 per cent.) there is present in the cylinder a mass of CO_2, H_2O, and inert gas in complete chemical equilibrium. In the 9/1 mixture this state is, of course, attained very much sooner. The difference in the behaviour of the weak and strong mixtures is wholly due to the very slow propagation of flame in the former; in a 9/1 mixture the flame seems to travel about 10 times as fast as in the 12/1 mixture.

In the 12/1 mixture maximum pressure is about 50 lbs. above atmosphere, and is reached about 2·5 seconds after the passage of the spark. During nearly half that time, at least half the area of the vessel has been in contact with the flame. It seems probable, therefore, that the proportionate loss of heat before the attainment of maximum pressure in a weak mixture is considerably greater than in a strong one. In other words, if the explosion were adiabatic the ultimate pressure produced would exceed the maximum actually observed by a greater proportion in the case of the weak than in that of the strong mixture, and this mainly by reason of the greater percentage loss of heat.

Description of Apparatus and Records.

Fig. 1 shows a section of the vessel along its axis, with its principal dimensions. A is the sparking point—nearly at the centre of the vessel. B, C, and D are three platinum thermometers; B, in the case shown, is close to the spark, C at a distance of about 30 cm., and D about 1 cm. from the walls of the vessel. Each thermometer consists of a coil of about 5 cm. of pure platinum wire, 0·001 inch diameter. The coil is hard-soldered to two thicker platinum wires sealed into the ends of glass tubes, and the thicker wires are soldered to stout copper leads. The copper-platinum junctions, being just within the tubes, are protected from the flame. Owing to this protection, and to the rapidity of the changes of temperature to be measured,

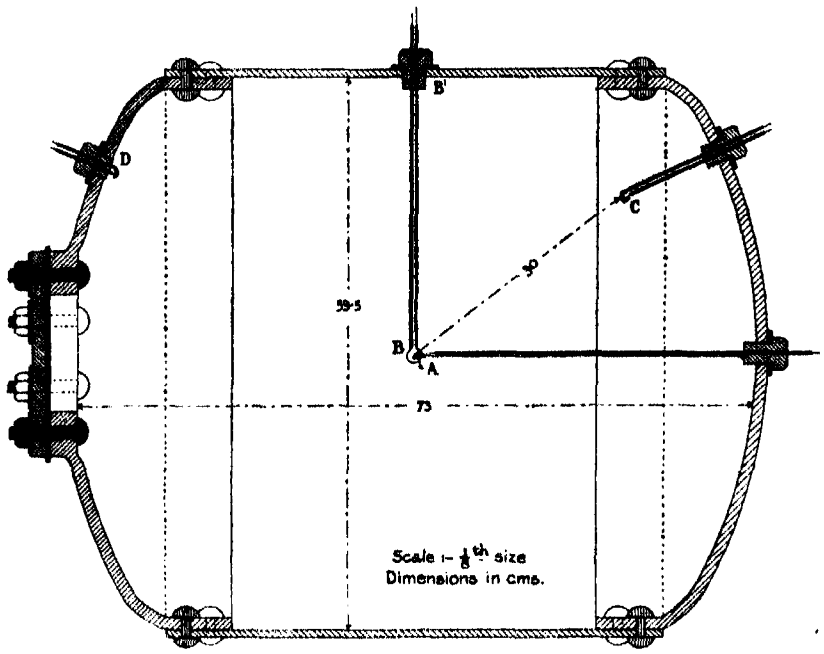


Fig. 1.

such changes are practically confined to the fine wire, and no compensating leads are necessary, nor is there any substantial thermo-electric effect. Each thermometer coil is placed in series with a storage cell and with a d'Arsonval galvanometer. The galvanometer has a stiff suspension of phosphor-bronze wire, giving a period of oscillation of from 1/50 to 1/30 of a second. The mirror of the galvanometer throws the image of a fine hole, illuminated by an arc-lamp, on to a revolving drum carrying a photographic film. The deflection of the galvanometer, when corrected for inertia effects, gives the current flowing, which is inversely proportional to the resistance of the circuit. From the resistance so obtained is deducted the resistance in the cold, and the difference is the rise in the resistance of the fine wire. Hence, knowing the resistance of that wire when cold, we can calculate its rise of temperature. Ordinarily two thermometers were in use at once, each with its galvanometer, and a record was thus obtained, on the same drum and in the same explosion, of the changes of temperature at two different points of the vessel.

A record of pressure was taken on the same drum. The indicator was of simple construction, consisting of a steel piston, which was forced by the pressure against a piece of straight spring held at the ends. The displacement of the spring tilted a mirror about a fulcrum, and the mirror cast an image of the above-mentioned fine hole on the moving film. A detailed

description of the indicator is unnecessary; it will suffice to say that it had a natural period of about 1/300 of a second, and so was able to follow very rapid changes of pressure.

Before making an experiment steam was blown into the vessel, so that the air should be as nearly as possible saturated with moisture. As will be seen later, the quantity of moisture present has some influence on the temperature reached, and there is some uncertainty from this cause, as one could not be sure that the saturation was complete. The gas was measured in by exhausting the vessel until the pressure was 9/10 of an atmosphere and then admitting the gas. From four to six hours were allowed for mixing, and the completeness of the combustion was tested in a few cases, and found satisfactory by observing the fall of pressure due to condensation of the steam formed in the explosion. The galvanometers were calibrated at the time of the experiment by observing the deflections produced when known resistances were placed in the circuit instead of the thermometer coils. Fig. 2 is a facsimile of one of the records obtained. One revolution of the drum, equal to the length of the diagram, is about 1·15 seconds. The pressure record, which is marked A, needs no comment. It may, however, be observed that the indicator shows traces of very rapid oscillation of pressure just before, and for a short time after, the maximum. These oscillations have a frequency of about 1000 per second, which is roughly the frequency of the note heard when the explosion takes place. Having regard to the much longer period of the indicator, they are evidence of a violent state of motion within the vessel, due, no doubt, to the setting up of an explosive wave shortly before maximum pressure is reached. The curve marked B is a record of the temperature at the centre of the vessel, being traced by thermometer B (fig. 1). B_0 is zero line for this curve, and is traced on the film immediately after the explosion by disconnecting the thermometer circuit. The current flowing in this circuit is, therefore, after allowing for inertia effects, proportionate to the ordinate of curve B, reckoned from the zero line B_0, and the resistance of the circuit is inversely proportional to that ordinate. It will be observed that the current, after remaining at the value corresponding to atmospheric temperature, suffers a sudden diminution at the point B_1, the galvanometer being thrown into violent oscillation thereby. This is, of course, due to the spread of the flame to the thermometer coil; and, as the coil is within 2 cm. of the ignition spark, B_1 marks approximately the initiation of the explosion.

Tracing the variations of the ordinate of curve B it will be noticed that its mean value, when the oscillations are smoothed out, remains nearly constant after the first rapid fall, corresponding to a very slow increase in the

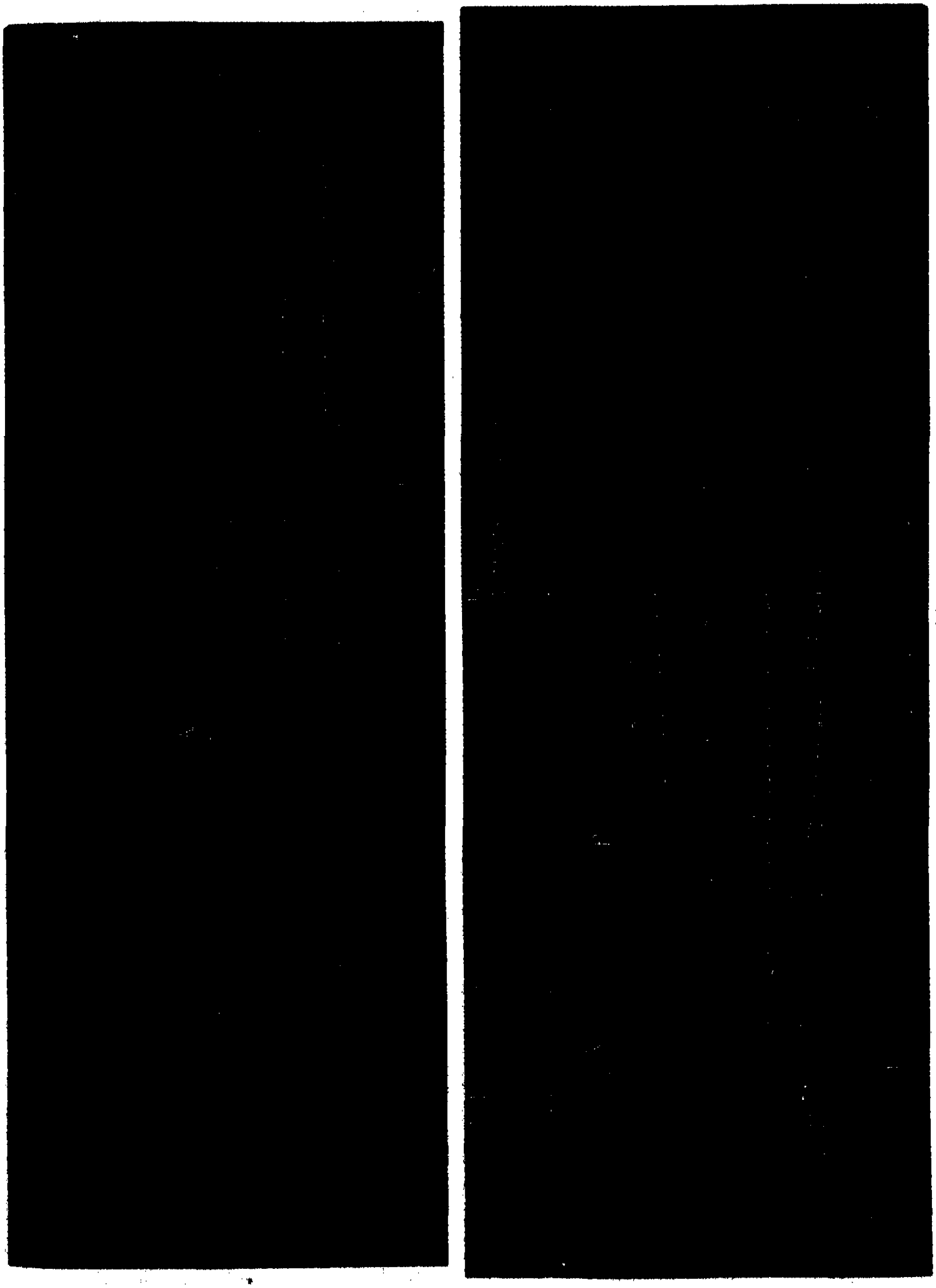


FIG. 2.—Air/Gas = 9.

temperature of the wire. It then diminishes owing to the rise of temperature produced by the adiabatic compression, until finally at the point B_2 it falls suddenly to zero. This point which is slightly before the maximum pressure, corresponds to the melting of the wire. The temperatures corresponding to the successive ordinates are plotted on fig. 3. Turn now to the line marked D. This is a record of the current in, and the temperature of, thermometer D (fig. 1) which is placed about 1 cm. within the wall of the vessel. The galvanometer in this case has a stiffer suspension and a lower

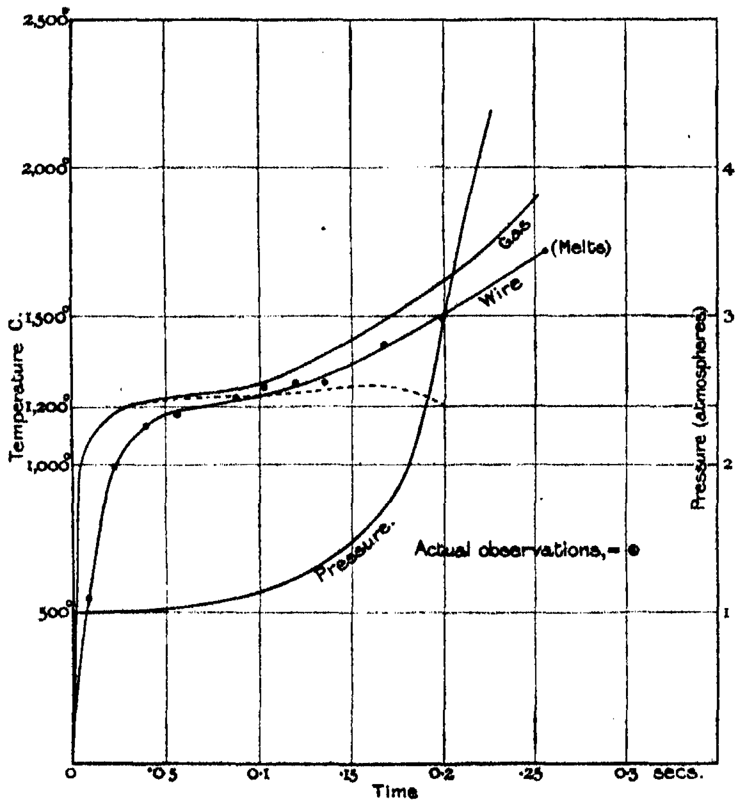


Fig. 3.

period of oscillation, and is therefore less sensitive. D_0 is the zero line for this record. It will be noticed that at the point D_1 the ordinate of this curve has begun gradually to diminish, showing the slow increase of resistance due to the adiabatic compression produced by the expansion of the ignited gas in the centre of the vessel, and it continues until the point D_2, where there is a sudden rise in resistance. This sudden rise is due to the flame reaching the thermometer coil and marks approximately the complete

spread of the flame through the vessel. The minimum ordinate and maximum temperature at D are reached at the moment of maximum pressure or very shortly afterwards. There is no pause in the rise of temperature followed by a more rapid rise, such as was observed in the case of B. The reason of this is, of course, that there is here no appreciable rise of pressure after combustion.

The simplest way of correcting for the inertia of the galvanometer is to smooth out the oscillations by taking the means of successive maxima and minima. This method of correction is satisfactory if the current does not vary very much in the course of an oscillation. Applying it to the curve B we get the following table :—

Table I.

Time (secs.).	Mean ordinate (mm.).	Resistance (ohms).	Rise of resistance (ohms).	Temperature (° C.).
0·008	22·2	22·05	12·4	560
0·024	16·2	30·3	20·7	995
0·041	15·0	32·7	23·1	1135
0·057	14·8	33·1	23·5	1165
0·074	14·8	33·1	23·5	1165
0·09	14·4	34·0	24·4	1225
0·107	14·2	34·5	24·9	1260
0·123	14·1	34·7	25·1	1275
0·140	14·1	34·7	25·1	1275
0·173	13·4	36·6	27·0	1400
0·26	Wire melts	—	—	1710

The time is reckoned from the point B_1 marking the commencement of combustion to a point half-way between the maximum and following minimum (or *vice versâ*) whose mean is shown in the second column. The third column is the total resistance of the circuit, obtained by dividing the second into 490—a number determined by substituting known resistances for the thermometer coil. The fourth gives the rise of resistance obtained by subtracting from the third column the resistance of the circuit other than the thermometer coil (3·2 ohms) and the resistance of the thermometer coil at 0° C. (6·37). The last column gives the temperature of the wire calculated in the usual way from the resistance. The temperature coefficient was determined to be 0·0038 between 0° and 100° C.: the correcting factor δ was given as 1·57 by the Cambridge Scientific Instrument Company who supplied the platinum wire.

The maximum temperature shown on curve D is about 1250° C., and occurs very nearly at the moment of maximum pressure. This estimate of

temperature, however, may be a good deal wrong, because the diagram, being on a smaller scale, cannot be measured so accurately as B.

The velocity of propagation of the flame was found by taking records in which thermometers B and C were used. In three different explosions the times taken to travel this distance (about 30 cm.) were respectively 0·19, 0·21 and 0·17 second. In another explosion a pyrometer was placed just inside the wall at the point B′. It was found that though this point is if anything rather nearer to the spark than C, the flame reached it 1/70 of a second later, a further illustration of the irregular manner in which the flame is propagated.

A large number of diagrams were taken with a weak mixture in which the proportion of air to gas was 12/1. The thermometers were placed in all sorts of positions. The general character of the results is indicated above. One diagram is reproduced in fig. 4. In this case one wire only was used, and it was placed about 15 cm. from the spark and vertically below it. It will be observed that at first the temperature rises very slowly. More than two seconds after ignition (at A) it is only about 210° C., and such heating as has then occurred is almost entirely due to adiabatic compression.* The flame now reaches it (A) and the temperature rises in 1/10 of a second to 1300° C. The pressure has now attained its maximum value (50 lbs. above atmosphere) and about 2½ seconds have elapsed since the spark passed. The temperature remains steady for a while and then falls; but there is no perceptible rise after the pressure begins to fall. In a large number of trials I was unable to discover any point at which inflammation occurred later than in the diagram here shown, so that it is probable that all the gas is ignited at the time of maximum pressure. Moreover, this must be nearly the last portion of the gas to be ignited, and here, if anywhere, the effects of slow combustion of gas already ignited would be perceptible in a rise of temperature after maximum pressure. In other words the gas is in chemical equilibrium within a very short time after maximum pressure.

The Specific Heat at Constant Pressure and the Velocity of Reaction.

Returning now to the record shown on fig. 2 with a 9 to 1 mixture, the temperature of the wire at the centre of the vessel is plotted in terms of the

* Owing to the heating effect of the current the wire starts about 90° hotter than the gas. The compression of the gas to 50 lbs. above atmosphere will cause its temperature to rise 160° C. The wire will then be hotter than the gas, but the difference will be somewhat less than 90°, since the heat to be dissipated is less because of the increased resistance. At temperatures of 1000° and over, the resistance is so great that the current in the wire can have but little effect upon its temperature.

time on fig. 3. The gas temperature always exceeds that of the wire, partly because of time-lag, and partly because of radiation. The gas temperature is shown on the same figure, being calculated from that of the wire by methods to be explained later. On the same figure is shown the rise of pressure.

The dotted curve shows the temperature of the gas corrected for compression, that is, it is the temperature which the gas would have had if it had remained at atmospheric pressure. In calculating this curve it is assumed that the relation between pressure and temperature in adiabatic compression is $\theta \propto p^{0\cdot 23}$. The selection of the exponent will be justified later; it corresponds to taking γ (the ratio of the specific heats) as 1·3.

It will be noticed that from 0·05 second to 0·2 second the temperature corrected for compression is within 30° of 1230° C., beyond this point the deviation becomes wider, but then the correction is no longer small, and the exponent of p diminishes as the temperature rises. Moreover a temperature of 1170° C. is reached in 0·02 second. We may take it then that the ultimate temperature produced by the combustion at constant pressure of the particular mixture used in this experiment is 1230° C., and that the combustion is complete within 5 per cent. in 1/40 of a second. The composition of the mixture is rather uncertain, but assuming that the gas has its average composition, and that,

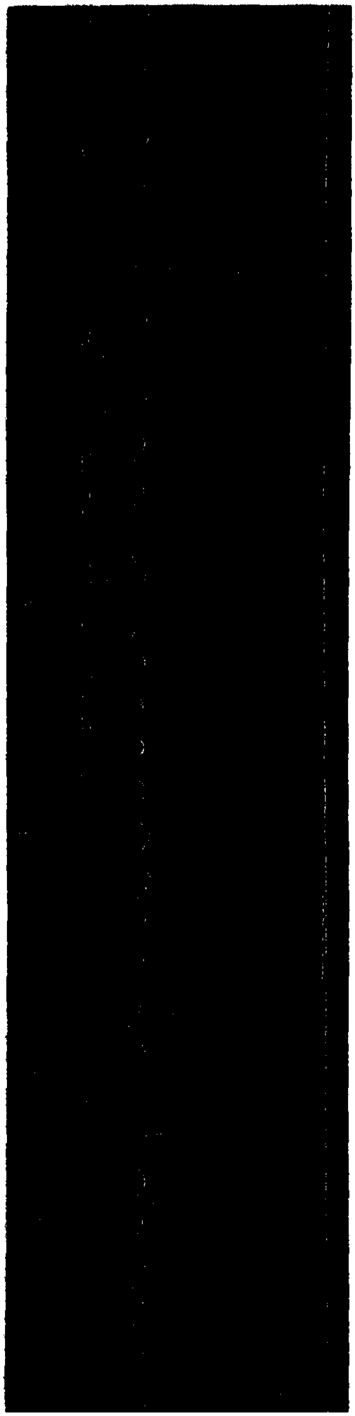


FIG. 4.—Air/Gas = 12.

[The successive portions of the pressure and temperature curves are numbered in the order of the corresponding revolutions of the drum.]

before explosion, both gas and air are saturated with moisture at 20° C. and 14·7 lbs. per square inch, the products of the combustion will be as follows:—

CO_2	0·56 cubic feet.
H_2O	1·53 „
N and O	7·60 „
Total........	9·69 „

assuming that the CO_2 and H_2O occupy their proper molecular volumes and the volumes being reckoned at 20° C.

The heat produced by the combustion when the gas is burned in a calorimeter is approximately 620 B. Th. U., the products being cooled to 20° C. Of this, however, about 60 B. Th. U. are to be ascribed to the condensation of the steam produced in the explosion. If the cooling were stopped at 55° C. (when condensation begins) only 550 B. Th. U. would be obtained, about 10 heat units being due to the cooling of the gases other than steam from 55° to 20°. Moreover in the burning as it actually takes place a certain portion of the heat is radiated from the surface of the flame.* The amount

* In the paper as originally written there was a note dealing with this point, in which I gave my reasons for supposing that the heat radiated could be neglected, and the specific heat was calculated on that assumption. Some remarks made by Professor Callendar at the discussion of the paper, however, have led me to look into the matter further, with the result that I must now admit the probability of a loss by radiation during the early stages of the explosion of 15 per cent. of the total heat then being generated. Professor Callendar stated that he had observed that an ordinary Bunsen flame radiates from 15 per cent. to 20 per cent. of the total heat of the gas used, which is a much greater proportion than I had thought possible; it is reasonable to suppose that the proportion of heat radiated from the flame at the centre of my vessel will at least be of the same order, though the flame is somewhat colder than the Bunsen flame. Among the reasons that I gave for neglecting the radiation was the fact that no rise of temperature, other than that due to the adiabatic compression of the gas surrounding them, was observed in any of the wires in the outer part of the vessel (*e.g.*, that at C, fig. 1), until they were actually in the flame. On working out the actual figures, however, I find that radiation from the flame could not certainly be detected by such rise of temperature unless it amounted to 15 per cent. of the heat generated, in which case it would just be apparent.

An estimate of the specific heat, unaffected by radiation errors, can be obtained from the explosion of a weak mixture, such as that giving the diagram of fig. 4. In this case the burning of the last portion of the gas to be ignited, which is that round about the thermometer coil, takes place in an inward direction, so that there can be but little radiation from the flame surface. The gas just before ignition has been compressed to 50 lbs. per square inch above atmosphere, and its temperature is very nearly that due to such compression, viz., 160° C. On combustion the temperature rises to 1300° C. The products of combustion of 1 cubic foot of gas in this case consist of about 2·1 cubic feet of steam and CO_2 and 10·8 cubic feet of N and O. The internal energy is the heat of

of this loss is quite uncertain; but it is improbable that it exceeds 15 per cent. If we assume that figure, there remain 470 B. Th. U. as the heat evolved in cooling the products at constant pressure from 1230° to 55° C. The same volume of air cooled through the same range would evolve 370 B. Th. U. Thus the average volume specific heat of the products is 1·3 times that of air. At constant volume the ratio would be 1·4, taking γ for the products as 1·3. If, on the other hand, we assume that the radiation can be neglected, the ratio of the specific heats of air and products is 550/370 or about 1·5. I think it is fairly certain that the true value is between 1·3 and 1·5, and probable that it is near the lower figure. This, of course, is the average value; at the upper temperature the ratio will be a good deal greater. At 55° C. the specific heat of the products is about 1·05 times that of air. Of the heat evolved in cooling, 290 B. Th. U. are accounted for by the nitrogen and oxygen present. The balance of 180 B. Th. U. (assuming the 15 per cent. loss) is the heat capacity of the 2·1 cubic feet of CO_2 and H_2O. The average volume specific heat of these gases is accordingly about 2¼ times that of air over the range 55° to 1230° C. It is obvious that the uncertainties in the amount of radiation, the composition of the gas, etc., are such that this can only be regarded as a very rough approximation to the truth.

It is not difficult to measure the original diagram correct to 2/10 mm. and the ordinates in the second column are probably within that amount of the truth, and certainly within 4/10. The corresponding errors in the temperature, at the highest temperature measured, are about 35° and 70° C. respectively. The order of accuracy of the results is further shown in the following table, which gives the temperature found at corresponding times in six different explosions. The composition of the mixture is nominally the same in all cases except the second, in which the preliminary steaming was omitted, so that the gases were very much drier than in the other cases.

combustion (550 B. Th. U.) plus the heat required to raise the mixture to 160° C. before firing (63 B. Th. U); total, about 610 B. Th. U. evolved in cooling from 1300° to 55° C. Of this the N and O account for 427 B. Th. U., leaving 183 for the 2·1 cubic feet of CO_2 and H_2O. The capacity of heat for the latter is accordingly 2·16 times that of the same volume of air. If we take this figure and calculate from it the heat required to warm the products of the explosion of 1 cubic foot of gas and 9 of air from 55° to 1230° C., the result is 464 B. Th. U., whereas the total heat evolved in that explosion is 550 B. Th. U. The discrepancy may be accounted for by supposing a radiation loss of about one-sixth part; and if such loss be assumed, the specific heat of the products at constant pressure works out to about 1·3 times that of air.

The gas *within* the flame surface is of course unaffected by radiation, and its treatment must be sensibly adiabatic until convection currents come into play.

Table II.

July 19.*	Sept. 8.*	Sept. 11.*	Sept. 15.	Sept. 19.	Sept. 21.†	Mean.
1010°	1205°	995°	995°	1065°	965°	1000°
1110	1220	1135	1085	1085	1115	1130
1135	1260	1165	1115	1120	1200	1147
1200	1330	1165	1150	1200	1250	1190
1250	1330	1225	1200	1245	1230	1230
1225	1300	1260	1205	1195	1230	1220
1240	1300	1275	1185	1270	1230	1240

* Wire melted.

† Wire 10 cm. long, and further from spark.

In criticising this table it is to be remembered that the calorific value and composition of the gas were not determined from day to day, and the calorific value may have varied between limits differing by 5 per cent. Moreover, the amount of water vapour present must also have varied somewhat. The combustion of 100 volumes of gas in 900 of air produces 133 volumes of steam, in addition to this there might be present (at 20° C. 760 mm.) anything up to 23 volumes of moisture before explosion, according to the dryness of the gases. The high temperatures shown on September 8 may be explained by the fact that there the gases were nearly dry, whereas in the other cases they were nearly saturated. This result also serves to show the considerable influence of the amount of moisture present—due probably to the high specific heat of steam at such temperatures. In calculating the mean temperatures in the last column, the results of September 8 are left out of account. The temperatures are those of the wire, uncorrected for radiation or time-lag.

The Ratio of Specific Heats.

In the adiabatic compression of any substance we have

$$\frac{dp}{d\theta} = \frac{c_p}{\theta\,(\partial v/\partial\theta)_p}. \tag{1}$$

We have here to deal with a mixture of gases, 79 per cent. of which is perfect gas. The remainder, CO_2 and H_2O, is not perfect, but its deviation from the gas law $pv/\theta =$ const. is dependent on dissociation, the effect of which is limited in amount to a reduction of the absolute density to two-thirds of its value at ordinary temperatures. If dissociation were complete the density of the mixture would become 10 per cent. less than at ordinary temperatures. In other words the characteristic equation is

$$pv/\theta = \mathrm{R}, \tag{2}$$

where R is a continuously increasing quantity, which at very high temperatures

may attain a value 10 per cent. in excess of its value at 50° C. At 1200° C. it is improbable that dissociation is complete, or that R has nearly reached the limit of its increase. Now differentiate (2):—

$$\left(\frac{\partial v}{\partial \theta}\right)_p = \frac{1}{p}\left\{\theta\left(\frac{\partial R}{\partial \theta}\right)_p + R\right\},$$

and we may now write (1) in the form

$$\frac{dp}{d\theta} = \frac{p}{\lambda\theta}, \quad \text{where} \quad \lambda = \frac{1}{c_p}\left\{R + \left(\frac{\partial R}{\partial \theta}\right)_p \theta\right\}, \qquad (3)$$

and is a quantity which changes slowly with c_p and R. The integral of (3) is $\theta \propto p^\lambda$ approximately. Now c_p has been shown by the experiments to be more than 25 per cent. greater at 1200° C. than at 50° C. It is unlikely that the term $R + \theta(\partial R/\partial \theta)_p$ increases in anything like so large a proportion, since R cannot increase by more than 10 per cent. We may, therefore, confidently expect a substantial diminution in λ as the temperature and pressure rise. Now for a perfect gas $\lambda = (\gamma - 1)/\gamma$, where γ is the ratio of the specific heats, and this relation is also nearly true for the mixture in question. At 50° C. the value of γ for this mixture is not accurately known, but it is probably not greater than 1·37, making λ something less than 0·27. It may be expected, therefore, that λ at 1200° C. is less than 0·27, and γ less than 1·37.

An inferior limit to λ at 1200° C. can be deduced from the experiments as follows: In the explosion described above, the wire melted at a pressure of 6·5 atmospheres. The temperature of the gas must then have been considerably above the melting point of platinum; in all probability it was 1900° C.* At a pressure of one atmosphere the temperature of the gas was 1230° C. Calculating from the equation $\theta \propto p^\lambda$ it follows that the average value of λ between 1200° C. and 1900° C., is 0·2. Since λ is a diminishing quantity, its value at 1200° must be greater than the average value between these limits, viz., greater than 0·2.

We may, therefore, assert with a high degree of probability that the value of λ for this mixture is between 0·2 and 0·27 at 1200° and that the ratio of the specific heats at that temperature is between 1·25 and 1·37. For the purpose of correcting the temperatures in the neighbourhood of 1200° C., the value 0·23 was assumed for λ, and for a small correction this is no doubt near enough to the truth.

The Measurement of Temperature.

The temperature of the gas differs from that calculated from the resistance of the wire from four causes, the effects of which must be considered:—

* See the section "Measurement of Temperature," below.

(1) The law connecting the resistance of platinum and its temperature has not been experimentally verified beyond 1000° C. Up to that temperature, however, it has been found that the temperatures given by the platinum and gas thermometers agree within about 10° C. I found that a sample of platinum wire whose purity is indicated by its temperature coefficient—0·00388—melted when its temperature as shown by its resistance was 1670°. The real temperature must have been slightly under 1710°. It is probable that extrapolation up to 1500° C. will be within the general order of accuracy of these experiments.

(2) The ends of the wire are colder than the middle because of conduction. As the diameter of the wire is only 1/2000 of its length, it may be expected that this correction will not be important. Suppose that the temperature falls at the end of the wire by 1000° in 1 mm., and that the heat conductivity of the platinum is the same as when cold, viz., 0·08, then the amount of heat conducted away per second by the wire 0·001″ diameter will be $\frac{10000}{200000} \times 0{\cdot}08 = 0{\cdot}004$ calorie per second. Now the heat supplied to the wire by the gas per millimetre length is shown below to be about 0·0035 calorie per second for every 100° by which the gas is hotter than the wire. A comparison of the two magnitudes shows at once that at a distance of 1 mm. from the end the temperature of the wire must have become sensibly uniform. This inference has been verified by comparison of the temperatures shown by wires of very different length (ranging from 3 to 10 cm.) under the same circumstances.

(3) The temperature of the wire lags behind that of the gas, when the latter is changing rapidly. To test the effect of this, wires of two different thicknesses—1/1000 and 2/1000 of an inch respectively—have been placed as close together as practicable, and their temperatures compared in the same explosion.* The result is shown on fig. 5, and was confirmed in several experiments. Both wires were within 5 cm. of the spark, so that the rise of temperature in the thin wire is similar to that shown on fig. 1. It melted shortly before the attainment of maximum pressure. At the point A the temperature of the thick wire is rising at the rate of 5000° per second. At the same moment the temperature of the thin wire is rising about 1300° per second. Since the thick wire has four times the mass of the thin, it is receiving heat about 15 times as fast. Now, the rate at which a very fine wire receives heat from a gas, by conduction, is almost independent of its diameter and, if this be true of our two wires, the difference of

* A somewhat similar method has been proposed by Professor Callendar for determining the radiation correction, 'Proc. Inst. Mech. Eng.,' 1901.

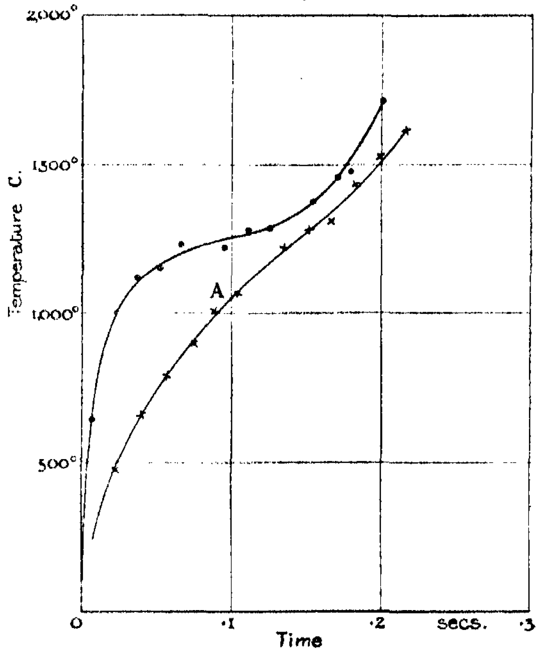


FIG. 5.

temperature between the thick wire and the gas must be 15 times as great as that between the thin wire and the gas, from this cause. Since the thick wire is here 200° hotter than the thin, the gas must be about 15° hotter than the thin wire—for our purpose an insignificant amount. It is possible, however, that convection currents play a more important part in giving heat to the wire than the kinetic theory in its simplest form would indicate. The effect of such currents is proportional to the surface of the wire, and the thick wire must in consequence receive heat at a greater absolute rate than the thin for the same temperature difference between wire and gas. If convection currents were the predominant factor, the thick wire would receive heat twice as fast as the thin. In this case the thin wire would be about 30° C. colder than the gas at the point A. This may be regarded as a superior limit to the error due to time-lag at this point. Taking the assumption that the two wires receive heat at the same rate for the same temperature difference, it follows that when the temperature of the thin wire is rising 1000° per second it must be about 12° colder than the gas, and this is the constant used in correcting the temperature in fig. 3.

As regards the use of platinum thermometers in general for the measurement of gas-engine temperatures, the comparison of thick and thin wires

here described shows that if the temperature is changing at the rate of 1300° per second, a wire 0·002″ diameter will be at least 200° hotter or colder than the gas. In a gas-engine running at 120 revolutions per minute the mean temperature falls from 1600° to 1000° C. in about ¼ second, so that wires of this diameter give untrustworthy measurement of temperature, except, perhaps, at the extreme end of the stroke.*

(4) One other source of error remains to be considered, namely, that due to radiation. In consequence of this the wire must always be somewhat colder than the gas, since it has to receive heat at the same rate as it loses heat by radiation. The correction necessary for this may be deduced from the comparison of thin and thick wires described in the last paragraph. At the point A the thick wire is getting hotter at the rate of 5000° C. per second. Its diameter is 0·002″, hence assuming that the specific heat of platinum at 1000° C. is 0·04, the capacity for heat of the wire is $1·72 \times 10^{-5}$ gramme-water unit per centimetre. It is therefore receiving heat at the rate of 0·086 calorie per second, plus any loss due to radiation. It will presently appear that the latter item is small compared with the first. The gas at this point has a temperature of 1250° in round numbers. It follows that with a temperature difference of 250° C. between gas and wire, the latter will receive heat at the rate of 0·086 calorie per second per centimetre.

Now the heat radiated by a platinum wire at 800° C. has been found by Bottomley to be about 0·2 calorie per second per square centimetre of surface.† Petavel has extended Bottomley's investigation by comparing the radiation at various temperatures up to 1700° C.‡ Combining the results of the two researches it appears that a wire at 1200° C. will radiate 1·1 calories per second per square centimetre. The wire 0·001″ diameter therefore radiates 0·0088 calorie per second per centimetre of length. To supply this amount of heat the gas must be 25° hotter than the wire. This is the correction applied in fig. 3 in addition to that for time-lag.

The conductivity of the gas increases with the temperature. According to the kinetic theory in its simplest form, the conductivity should be proportional to the square root of the absolute temperature; but it is found, in fact, to vary more rapidly than this, though always less than in proportion to the temperature. So far as I am aware, there are no experimental data for temperatures above 1200° C., and I assume that the conductivity then varies as the square root

* The smallest wire used by Professor Burstall was 0·0015″ diameter. The error due to time-lag would be half that of a wire 0·002″ diameter, but is still of the order of 200° C.

† Bottomley, 'Phil. Mag.,' vol. 49.

‡ Petavel, 'Phil. Trans.,' A, vol. 191.

of the absolute temperature. The temperature difference necessary to maintain the heat lost by radiation is then proportional to the rate of loss divided by such square root. The resulting values of the difference for wire 0·001″ diameter, based on Petavel's determination of the amount of radiation, are plotted on fig. 6. In the explosion of a 9/1 mixture, the results of which

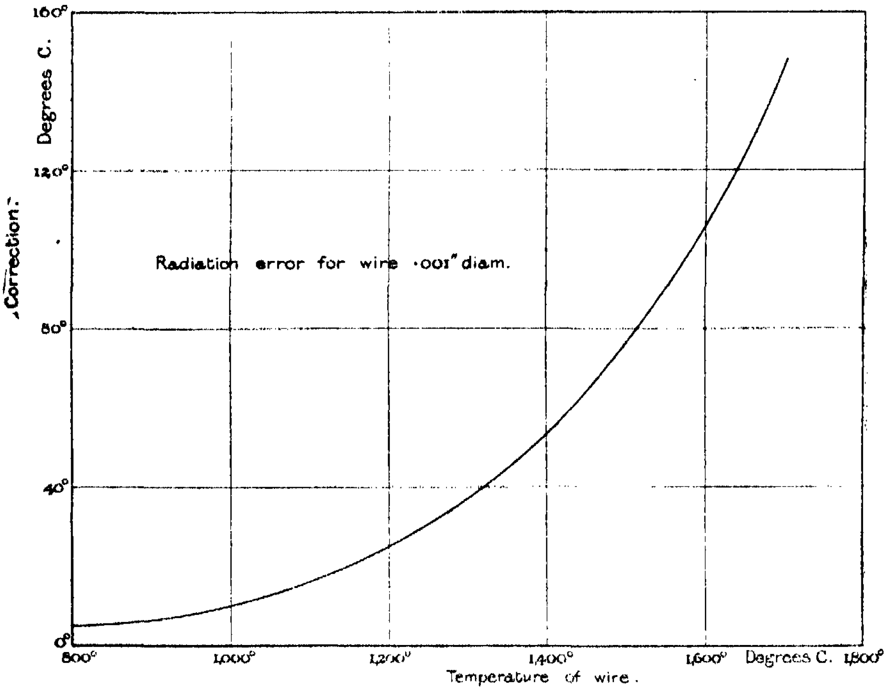


FIG. 6.

are shown on fig. 2, the maximum temperature reached by the wire is somewhat above its melting point—say 1750° C. At this point the error is 145° C., and the gas temperature 1900° C. If the conductivity varied as the temperature the error would be 120°, so that it makes very little difference which law is taken.

The radiation correction is proportional to the diameter of the wire. At 1000° C., with a wire 0·002″ diameter, it is 25° C., or only about 1/10 of that due to time-lag at the point A (fig. 5).

On the "Suppression of Heat" in Gaseous Explosions.

The experiments described above were undertaken largely with the object of finding the cause of the so-called "Suppression of Heat" in explosions. It has long been known that the maximum pressure reached in the explosion

of coal-gas or hydrogen, and air, is less than two-thirds of that which would be attained by an equal volume of air on the addition of the heat of combustion. A similar phenomenon appears in gas-engine indicator diagrams. In most such diagrams the expansion line is somewhat above the adiabatic expansion curve for air, though it is known that much heat is being lost during expansion. But though the fact is well established, there is still much controversy about the cause.

Confine the attention for the moment to closed-vessel explosions, such as those described in this paper, in which the mixture is strictly homogeneous before ignition. At the time of maximum pressure the rate of loss of heat to the walls is just equal to the rate at which the chemical energy of the gas is being converted into thermal energy. This statement requires some qualification in the case of strong mixtures (a point which is dealt with below), but is probably very nearly true of weak mixtures, which possess most interest from this point of view. It follows that at the moment of maximum pressure the gas has not quite attained chemical equilibrium. At some time after maximum pressure equilibrium is reached for practical purposes, and the gas then consists of a mixture of CO_2, H_2O, and inert gas, whose internal energy is all thermal energy and is equal to the heat of combustion less the heat lost up to that time. Messrs. Mallard and Le Chatelier and others assumed that this state was reached within a very short time of maximum pressure; and they utilised their records of explosion pressures to deduce the specific heat of the products of combustion. From a study of the curves of pressure during cooling they calculated the loss of heat during burning, and so determined the internal energy of the burnt mixture at or about the time of maximum pressure. This was assumed to be all thermal energy, and so the capacity for heat was determined. The calculation of the loss of heat is open to serious criticism, since the state of the gas during cooling is very different from its state while burning is in progress. But at the time of maximum pressure the correction is still a small one, and the possibly considerable error in it does not affect their broad result, which was that at 1200° C. or over, the specific heat of the products of combustion in the gas-engine must be much greater than that at ordinary temperatures.

It has, however, been urged by Dugald Clerk* that the assumption that chemical equilibrium is almost simultaneous with maximum pressure is unjustifiable. The gas may have failed to attain equilibrium in either of two ways. Firstly, the flame may not have spread to every part. There may be discrete portions of gas which are still in their pristine state and in which combustion is actually not started. If I understand their views

* 'Inst. C. E. Proc.,' vol. 85 (1885), p. 1.

correctly, all, including Mr. Clerk, are agreed that this is not the fact, but that at the time of maximum pressure, if not some time before, combustion is fairly started everywhere in the vessel. My own experiments support this view. Mr. Clerk, however, suggests the second alternative, namely, that though the reaction is initiated everywhere, it is not complete. On the analogy of other reactions we know that some interval must elapse between the beginning and the completion of the combination of these gases at any point. Mr. Clerk contends that in the case of a weak mixture the interval is a long one—that in places heat is still being produced by the transformation of chemical energy long after the time of maximum pressure, and this though every bit of gas is inflamed before that time; and he seems to consider that even in strong mixtures a considerable proportion of the internal energy is in chemical form at the moment of maximum pressure. He suggests that the "suppressed heat" is to be largely, if not entirely, accounted for in this way.

My experiments do not support such a view as this; they appear to me to prove that even in the weakest mixtures combustion, when once initiated at any point, is almost instantaneously complete. Moreover, they show that the specific heat of the products is very much greater at high temperatures than at low, and the extent of the difference seems to justify the view that it is the main reason of the so-called "suppression of heat." It may be added that this rise in the specific heat is consistent with direct observations of that constant for CO_2, which have been made up to about 800° C., and prove that it increases considerably.

It was suggested above that in the case of strong mixtures maximum pressure does not necessarily mark equality between the rate of loss of heat and the rate of transformation of chemical energy into heat. At the moment of maximum pressure the gas, in addition to possessing untransformed chemical energy and heat energy, is in violent motion, and, moreover, it is very far from being in thermal equilibrium. The kinetic energy of the gas is being changed by viscosity into thermal energy, and, quite apart from want of chemical equilibrium, this might cause the pressure to be stationary, and in actual fact must certainly tend towards that result. The fact that the gas is not in thermal equilibrium would not have any effect upon the pressure observed if it were a perfect gas having a constant specific heat; in other words, the internal energy of a quantity of unequally heated perfect gas is dependent only on its pressure and not on the distribution of temperature. The specific heat of the products of combustion in an explosion is, however, far from constant, being greater at high temperatures than at low, and it is easy to see that the result of this is to make the energy of an unequally

heated mass of such products greater than that of the same quantity at the same pressure when the temperature is uniform. Thus, if loss of heat to the walls were arrested at the moment of maximum pressure after an explosion of a strong mixture there would be a further rise of pressure, due solely to the attainment of thermal equilibrium. I have shown that the differences of temperature amount to 500° C., at least, in a 9 to 1 mixture; in the present state of our knowledge it is impossible to say how much effect the equalisation of these differences might have upon the pressure, but that it would cause some rise there can be no question.

But though I consider that Messrs. Mallard and Le Chatelier and their followers were probably right in supposing that the gas was in chemical equilibrium at the time of maximum pressure, or very shortly after, it seems to me that only a rough approximation to the specific heat can be obtained by a study of explosion pressure records only. The uncertainty as to the loss of heat, the great differences of temperature between one part and another, and the violent motion of the gas all conspire to make the results inaccurate; and if the pressure be observed at so long a time after maximum pressure that the gas may be taken to be at rest and in equilibrium, the loss of heat will have become so great as to make the results wholly untrustworthy.

The circumstances of explosion in the gas-engine cylinder are, of course, somewhat different from those obtaining in the closed vessel, where the gas before ignition is homogeneous and at rest. In the gas-engine the gas and air are usually introduced simultaneously at a high velocity, and the engine is designed so as to ensure, as far as possible, the thorough mixture of the incoming streams. Nevertheless, there is a possibility that the mixture is not quite complete, since from the nature of the case it depends almost wholly upon mechanical agitation and not upon diffusion. The mixture must certainly be homogeneous in the sense that a sample of, say, 100 c.c., from whatever part of the cylinder it be taken, will show the same composition. On the other hand, it is probable that the composition, as shown by a sample very small compared with the general dimensions of the vessel, and yet immensely great compared with molecular dimensions, will have very greatly varying composition according to the precise point from which it is taken. The structure of the gas may perhaps be of a streaky character, consisting of line of rich mixture embedded in a matrix of weaker mixture. Moreover, the gases in an engine are probably in turbulent motion at the time of ignition, and the movement of the flame will be somewhat different from that observed in a homogeneous mixture at rest. The agitation of the gas will certainly accelerate the spread of the flame; the want of homogeneity

may, perhaps, retard the combustion at any given point. It is quite unnecessary, however, to suppose that these influences have any substantial effect upon the gas-engine indicator diagram in order to explain its peculiarities. These follow at once from the fact, which I think may be considered as established, that the specific heat of the working substance is much greater than that of air, and from the very slow propagation of flame through a weak mixture.

When a fairly rich mixture is exploded in the gas-engine cylinder (say, air/gas$=9/1$), the ignition being slightly before the end of the compression stroke, the diagram shows a very sharp rise of pressure, followed at once by the falling curve of the expansion stroke. The diagram is almost identical with that which would be given by the sudden addition of the heat of combustion (less a small percentage of loss) to the mixture of CO_2, H_2O and inert gas, which results from the combustion, followed by expansion with some loss of heat to the walls. The internal energy of such a mixture at 1500° C. is about one and a-half times that of the same volume of air at the same temperature and pressure; hence the rise of temperature on explosion will be about two-thirds of that which would have taken place if the working substance had been air instead of the mixture referred to. Moreover, the ratio of the specific heats of the mixture between 1500° and 1000° C. is something less than 1·3. The expansion line will therefore, if adiabatic, lie above the line $pv^{1 \cdot 3}$ constant. There is no necessity at all to suppose that in this case we have in the cylinder anything but a mixture of CO_2, H_2O, and inert gases in complete chemical equilibrium for at least nine-tenths of the stroke. The natural rate of propagation of flame through such a mixture is high enough to fill the compression space before the piston has sensibly moved; and the time required for this process will be still further reduced by the motion of the gas and (probably) by its high temperature before ignition.

When a very weak mixture is used, the form of the diagram is so far modified that the maximum pressure occurs later in the expansion stroke, the curve corresponding to which first rises and then falls instead of falling for practically the whole of its course. This is exactly what would happen in the explosion of a homogeneous weak mixture, the volume of which is made to increase rapidly while the flame is spreading. Plainly in such a case the rise of pressure due to the spread of the flame might be balanced by the fall due to increase of volume. In the gas-engine the volume increases slowly at first, and the pressure then rises at an increasing rate as in the closed vessel. As the middle of the stroke is approached the increase of volume gets more and more rapid, until (at the moment of maximum pressure) the spread of the flame is just able to keep pace with it. Then the

piston slows down, and the flame usually overtakes it, and fills the vessel before the end of the stroke. Occasionally, however, there may still be unburnt gas present at the moment of release. Such extremely weak mixtures are, of course, not of much practical importance, because the gas is burnt in a very uneconomical way, much of it being ignited at a low compression.

It would seem that in all cases of importance in gas-engine practice the working substance may be treated as a mixture of CO_2, H_2O, and inert gas, in chemical equilibrium to which the heat of combustion, less a small percentage of loss, is added at the beginning of the stroke. The slowness of propagation of flame will cause the attainment of equilibrium to be more or less delayed, especially in high-speed engines; but the efforts of designers will naturally be directed to timing the commencement of the process and hastening its completion, so that every part shall be ignited under the best circumstances, that is, when the compression is a maximum. The next step in the development of gas-engine theory must be to ascertain the properties of this working substance, viz., its internal energy as a function of its temperature and pressure.

I wish to express my indebtedness to Professor Ewing for some valuable criticisms; and I have to thank Messrs. G. B. Ehrenborg, of Christ's College, Cambridge, and W. N. Duff, of Trinity College, for assistance in the experimental part of the work.

APPENDIX.

Composition of Cambridge Coal-gas.

	Per cent. by volume.	O required.	Steam.	CO_2.
H	47·2	23·6	47·2	
CH_4	35·2	70·4	70·4	35·2
Heavy hydrocarbons	4·8	22·6	16·0	14·4
CO	7·15	3·6	—	7·15
N	5·4			
Other gases	0·25			
	100·0	120·2	133·6	56·75

The second column shows the volume of oxygen required for complete combustion; the third and fourth, the resultant volumes of CO_2 and H_2O. The analysis was kindly given to me by Mr. Auchterlonie, Engineer and Manager to the Cambridge Gas Company.

The composition of the "heavy hydrocarbons" is somewhat uncertain. One hundred volumes of gas require 576 volumes of air for complete combustion. One hundred volumes of gas burnt in 900 of air give about 133 volumes of steam, 57 of CO_2, and 780 of inert gases; assuming that there is no dissociation.

On Methods whereby the Radiation of Electric Waves may be mainly confined to Certain Directions, and whereby the Receptivity of a Receiver may be Restricted to Electric Waves Emanating from Certain Directions.

By G. Marconi, LL.D., D.Sc.

(Communicated by Dr. J. A. Fleming, F.R.S. Received March 15,—Read March 22, 1906.)

This Note relates to results observed when for the usual vertical antenna employed as radiator or absorber in wireless telegraph stations there is substituted a straight horizontal conductor placed at a comparatively small distance above the surface of the ground or water.

When an insulated horizontal wire, AB, such as is shown in sketch 1, is connected at one end to a sphere of a spark gap, the other sphere of which

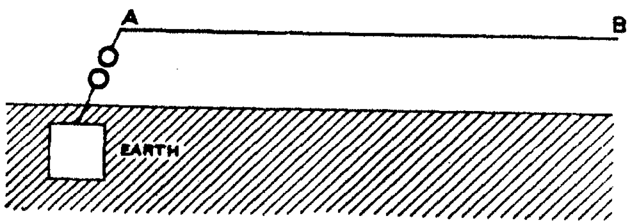


FIG. 1.

is earthed, and sparks are caused to pass between the spheres, it will be noticed on investigating the space around such an oscillator that the radiations emitted reach a maximum in the vertical plane of the horizontal wire, AB, and proceed principally from the end, A, which is connected to the spark gap, whilst the radiation is *nil*, or reaches a minimum, in directions which are approximately 100° from the direction in which the maximum effect occurs.

I have also noticed that any horizontal conductor of sufficient length

placed upon or at a short distance above the surface of the ground, and connected at one end through a suitable detector to earth, will receive with maximum efficiency only when the transmitter is situated in the vertical plane of the said horizontal receiving conductor, and in such a direction that the end connected to the detector and to the ground is pointing towards the transmitting station.

If, therefore, such a horizontal conductor be swivelled about its earthed end in a horizontal plane, the bearing or direction of any transmitting station within range of the receiver can be ascertained.

I have carried out a number of tests with transmitters and receivers having radiating or receiving antennæ or conductors arranged as follows:—

(1) Transmitting conductors consisting of horizontal wires, the radiations being received at a distance by means of the usual vertical wires suitably attuned.

(2) Both transmitting and receiving conductors consisting of horizontal wires.

(3) Transmitting conductors consisting of one or more vertical wires with or without capacity areas at top, such as have been generally employed in wireless telegraphy, the radiations being received by means of horizontal conductors.

At long distances I almost invariably used as a detector my magnetic receiver.* At shorter distances I utilised a Duddell thermogalvanometer,† by means of which it was possible to measure the root-mean-square values of the currents induced by the oscillations in receiving wires disposed in various positions relative to the transmitting conductors.

With arrangements such as are referred to in (1), the following tests have been carried out:—

1. *Transmitter.*—Horizontal wire 100 metres in length, direct excitation, spark length 2 cm., wave-length approximately 500 metres.

Receiver.—A vertical wire 8 metres in length, tuned to the period of the transmitter by means of a syntonising coil, and connected to a magnetic detector and to earth in the usual manner.

Results.—Signals quite distinct at 16 kiloms. in the vertical plane of the horizontal transmitting wire and in the direction of its earthed end; weak at 10 kiloms. in the same vertical plane, but in the reverse direction; inaudible at 6 kiloms. at right angles to the directions above mentioned.

2. *Transmitter.*—(At Mullion, Cornwall); consisting of horizontal conductor 150 metres in length, composed of four parallel wires about 3 mm. in

* See 'Roy. Soc. Proc.,' London, 1902, vol. 70, p. 341.

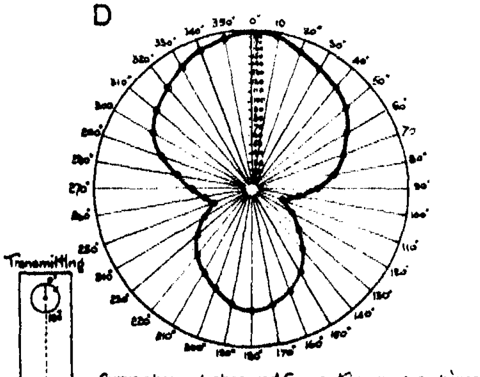
† 'Phil. Mag.,' 1904, vol. 8, p. 91.

diameter, placed 1·50 metres apart, supported at a height of 20 metres, and all connected to earth through the spark gap of an induction coil placed in a building on the ground; spark length about 2 cm.

Receiver.—At the Haven, Poôle (distance, 240 kiloms.); consisting of vertical wire 50 metres long, connected through a syntonising coil to a magnetic detector and to earth.

Results.—A movement of 15° of the plane of the transmitting conductor out of the right direction was sufficient to cause signals to become indetectible at Poole.

Polar diagram D (fig. 2) gives the values of the received current in micro-ampères, with conditions as marked under the diagram. The values of current in micro-ampères shown in each diagram are the mean of a considerable number of readings, the transmitted energy being kept as nearly as possible constant by means of a suitable interrupter applied to the sending induction coil.



Curve showing observed Current in micro-ampères at earthed end of Receiving Conductor under the conditions of Direction shown below:—
Transmitting Conductor:—Horizontal, rotating from 0° to 360° 1·5 metres above ground.
Receiving Conductor:—Vertical, fixed height 18 metres
Length of { Transmitting Conductor · 60 metres
Receiving Conductor · 18 metres }
Distance of Transmission · 260 metres

Fig. 2.

With the arrangement mentioned at (2) *i.e.*, both transmitting and receiving conductors horizontal, the following results, among others, were obtained:—

1. *Transmitter.*—Conductor 200 metres in length, supported at a height of 15 metres above ground; spark length about 2 cm.

Receiver.—Similar conductor supported 1 metre above ground, connected at one end to a detector and to earth as usual.

Results.—In the direction for maximum effect (as already explained) readable signals at 25 kiloms. At about 90° from said direction at 12 kiloms., nothing; in the same direction at 5 kiloms., weak signals.

2. *Transmitter.*—Consisting of four wires each 330 metres in length, separated from one another by a distance of 1·4 metres, supported at a height of 20 metres above ground and connected by means of a nearly vertical conductor to a spark producer; spark length, 3 cm.

Receiver.—Consisting of one wire 220 metres in length, covered with insulating material, placed on the ground and connected to the end nearest the sending station through a syntonising coil to a magnetic receiver and to earth.

Results.—When in the vertical plane of the transmitting antennæ, and in the best direction, weak but distinct signals were received at a distance of 160 kiloms.; at 45° from said direction and at 150 kiloms. distance nothing was received; at 25° from the best direction, and at 160 kiloms. distance, very weak signals were received.

The results over shorter distances are given by the readings obtained on the thermogalvanometer, and are shown in the polar diagrams E and B.

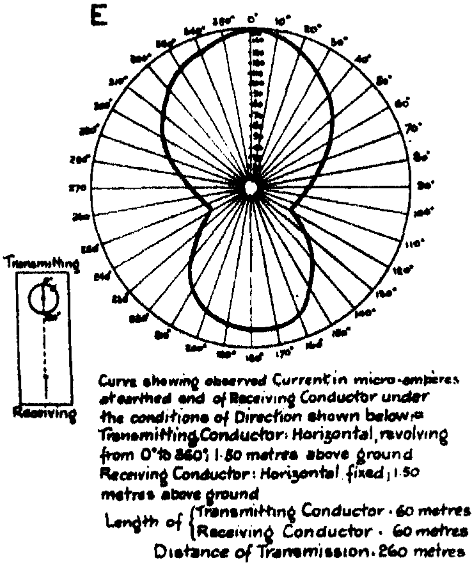
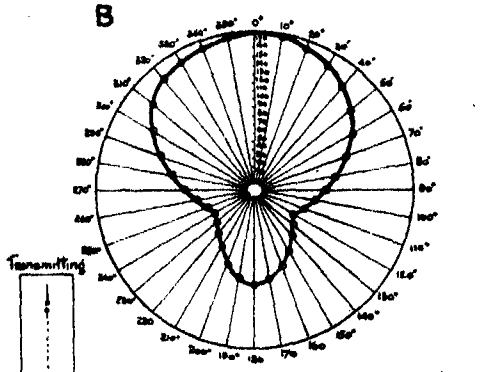


Fig. 3.

With arrangements such as are mentioned at (3), [*i.e.*, the transmitting conductor consisting of the usual vertical type and the receiving conductor horizontal, the following results among others merit attention:—



Curve showing observed Current in micro-amperes at earthed end of Receiving Conductor under the conditions of Direction shewn below.
Transmitting Conductor:- Horizontal, fixed 1·50 metres from ground.
Receiving Conductor:- Horizontal rotating from 0° to 360° 1·50 metres from the ground
Length of {Transmitting Conductor · 30 metres; Receiving Conductor 30 metres}
Distance of Transmission · 225 metres

FIG. 4.

At Clifden, Connemara, Ireland, by means of a horizontal conductor 230 metres in length, laid on the ground and connected at one end to a magnetic receiver and to earth, it is possible to receive with clearness and distinctness all the signals transmitted from the Poldhu station (situated 500 kiloms. distant) provided that the free end of the said conductor points directly away from the direction of Poldhu. No signals can be received if the horizontal wire at Clifden makes an angle of more than 35° with the line of direction of Poldhu.

The signals from the Admiralty station at Scilly can be received at Mullion, Cornwall (distance about 85 kiloms.) by means of a horizontal wire 50 metres in length, 2 metres above ground, provided said wire is placed in a radial position with respect to the sending station and with its free end pointing away from it. But it is unreceptive if placed so as to make an angle of more than 20° with the line of direction of the station at Scilly.

Some tests have also been carried out for the Admiralty in the vicinity of Poldhu in conjunction with H.M.S. "Furious." For this purpose eight horizontal wires 60 metres in length, supported at a height of about 2 metres, were arranged radially and made to converge in a small building situated in a field near Poldhu. These radial wires were so arranged as to divide the circle into eight equal sectors. By means of a suitable switch any one of the ends of these wires at the position where they converged together could be connected to earth through a magnetic receiver.

The wireless telegraph station on H.M.S. "Furious" consisted of an ordinary vertical wire aerial about 50 metres in length, connected to a suitable spark gap. The station on the ship transmitted at intervals, and the ship followed a course describing an arc of about 180° round Poldhu, keeping at distances varying up to 16 miles. By means of the horizontal wire arrangement, the bearing of the ship from Poldhu could be determined at any time by noting on which particular wire or wires the reception of signals was strongest, and also by observing which wires were non-receptive.

It was also found possible to receive simultaneously and without mutual interference different signals sent by means of oscillations of the same wave-length coming from the ship and from the Lizard wireless station (10 kiloms. away) whenever the ship was in such a position that its bearing from Poldhu made an angle of at least 50° with the bearing of the Lizard station.

For further values and curves of received current in receivers, I refer to Diagrams A, A′, C, C′.

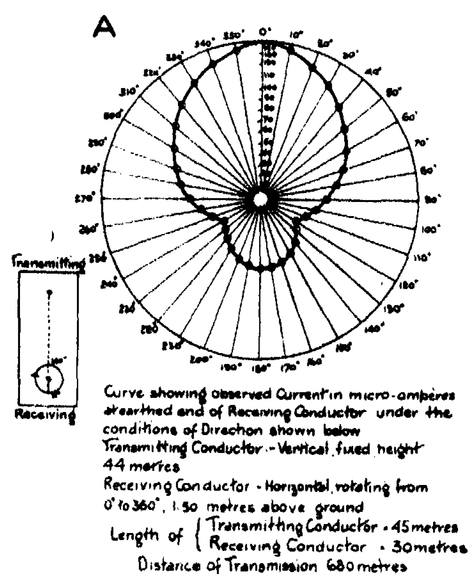


Fig. 5.

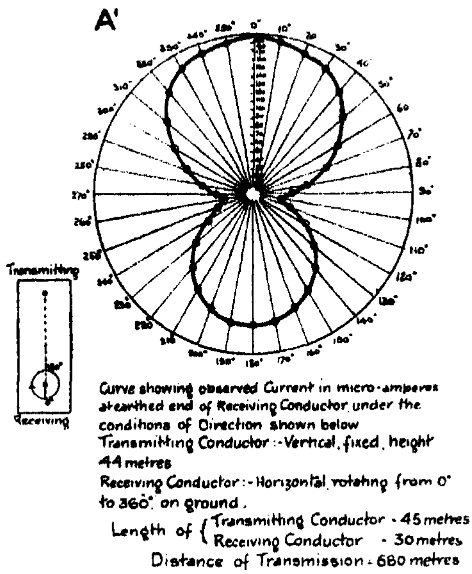

A'
Transmitting
Receiving
Curve showing observed Current in micro-amperes at earthed end of Receiving Conductor under the conditions of Direction shown below
Transmitting Conductor :- Vertical, fixed, height 44 metres
Receiving Conductor :- Horizontal, rotating from 0° to 360° on ground.
Length of { Transmitting Conductor - 45 metres
Receiving Conductor - 30 metres
Distance of Transmission - 680 metres

FIG. 6.

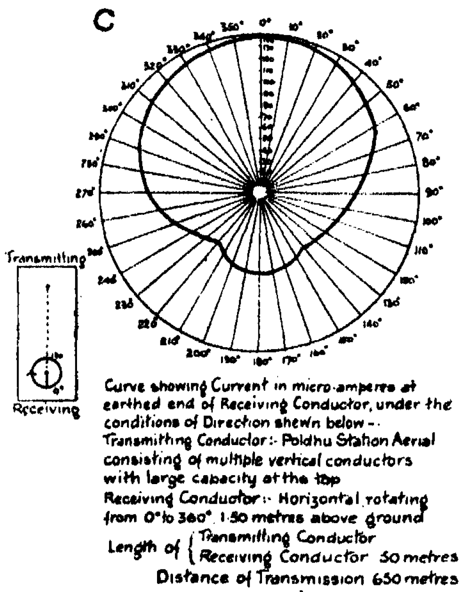

C
Transmitting
Receiving
Curve showing Current in micro-amperes at earthed end of Receiving Conductor, under the conditions of Direction shewn below -
Transmitting Conductor :- Poldhu Station Aerial consisting of multiple vertical conductors with large capacity at the top
Receiving Conductor :- Horizontal, rotating from 0° to 360°, 1·50 metres above ground
Length of { Transmitting Conductor
Receiving Conductor 50 metres
Distance of Transmission 650 metres

FIG. 7

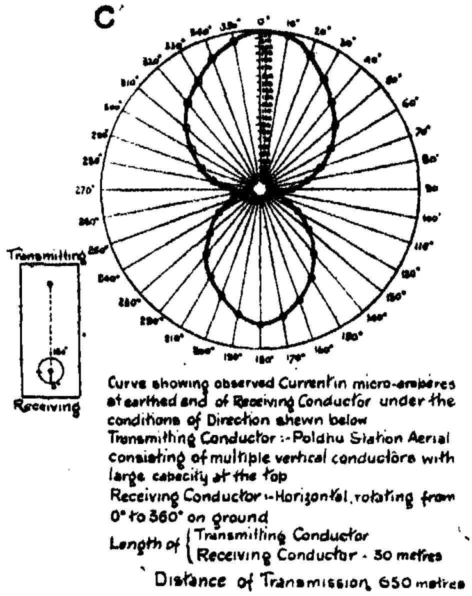


Fig. 8.

Referring generally to the results mentioned in this Note, I have observed that, in order that the effects should be well marked, it is necessary that the length of the horizontal conductors should be great in proportion to their height above the ground, and that the wave-lengths employed should be considerable—a condition which renders it difficult to carry out such experiments within the walls of a laboratory.

I have found the results to be well marked for wave-lengths of 150 metres and over, but have not been able to obtain as well-defined results when employing much shorter waves—the effects following some law which I have not yet had time to investigate. There also appears to be a decided advantage in so far as effects at long distances are concerned in utilising a directly excited radiating conductor—that is, an insulated conductor in which the high frequency oscillations are started by means of a suitable spark discharging it to earth or to another body, as was usual in my early forms of Hertzian-wave wireless telegraph transmitters.

If inductive excitation is employed, that is, if the oscillations are induced in the radiating conductor from another oscillating circuit, the comparative results in various directions appear to be in the same proportions as those noticed when using the method of simple excitation, but the distances over which the effects can be detected are much smaller at parity of the power employed at the transmitter.

I have noticed that the most advantageous length of the receiving horizontal wires, in order to obtain results at maximum distances, is about one-fifth of the length of the transmitted wave, if said wires are placed at a distance above the ground; but the receiving wires should be shorter if placed on the ground. It would be instructive to investigate more thoroughly the difference of the results and curves obtained by means of horizontal wires placed at different heights above ground, and also the effect of varying the length of said wires.

When using horizontal receiving wires arranged as described in this Note, I have often noticed that the natural electrical perturbations of the atmosphere or stray electric waves, which are generally prevalent during the summer, appear to proceed from certain definite directions which vary from time to time. Thus, on certain days, the receiving instruments when connected to wires which are oriented in such a way as to possess a maximum receptivity for electric waves coming from the south, will give strong indications of the presence of these natural electric waves, whilst on differently oriented wires the effects are at the same time weaker or imperceptible. On other days these natural electric waves may apparently come from other directions.

It would be exceedingly interesting to investigate whether there exists any relation between the direction of origin of these waves and the known bearing or direction of distant terrestrial or celestial storms from whence these stray electric waves most probably originate. A considerable number of observations would be necessary to determine whether there exists any relation between the bearing of storm centres and the direction of origin of these natural electric waves. I propose to carry out some further investigations on the subject.

I ought to explain that the experiments described in this Note were carried out during a period of many months, and that as other results achieved over greater distances coincide generally with those here described, I have not thought it necessary to make special reference to them.

I should also mention that the tests over short distances were carried out over practically flat country, whilst those over considerable distances took place over hilly country, such as the West of England, and in some cases partly across sea and partly across land.

On the Figure and Stability of a Liquid Satellite.

By Sir GEORGE HOWARD DARWIN, K.C.B., F.R.S.

(Received January 17,—Read February 8, 1906.)

(Abstract.)

More than half a century ago Edouard Roche wrote his celebrated paper on the form which a liquid satellite will assume when revolving, without relative motion, about a solid planet.* As far as I know, his laborious computations have never been repeated, and their verification and extension form a portion of the work contained in the present paper.

Two problems involving almost identical analysis, but very distinct principles, are here treated simultaneously. If we imagine two detached masses of liquid to revolve about one another in a circular orbit without relative motion, the determination of the shapes of each of them is common to both the problems; it is in the conditions of their secular stability, according to the suppositions made, that the division occurs.

The friction of the tides raised in each mass by the attraction of the other is one cause of instability. If now the larger of the two masses were rigid, whilst still possessing the same shape as though liquid, the only tides subject to friction would be those in the smaller body. It amounts to exactly the same whether we consider the larger mass to be rigid or whether we consider it to be liquid, and agree to disregard the instability which might arise from the tidal friction of the tides generated in it by the smaller body. Accordingly I describe secular stability in the case just considered as "partial," whilst complete secular stability will involve the tidal friction in each mass.

The determination of the figure and partial stability of a liquid satellite is the problem of Roche. It is true that he virtually considered the larger body or planet to be a rigid sphere, but in this abstract the distinction introduced by the fact that I treat the planet as ellipsoidal may be passed over. It appears that, as we cause the two masses to approach one another, the partial stability of Roche's satellite first ceases to exist through the deformation of its shape, and certain considerations are adduced which show that the most interesting field of research is comprised in the cases where the satellite ranges from infinite smallness relatively to the planet to equality thereto.

The limiting partial stability of a liquid satellite is determined by considering the angular momentum of the system, exclusive of the rotational momentum of the planet. This corresponds to the exclusion of the tidal

* 'Mém. Acad. Sci. de Montpellier,' vol. 1, 1847—50, p. 243.

friction of the tides raised in the planet. For any such given angular momentum there are two solutions, if there is any. When these two solutions coalesce for minimum angular momentum, we have found a figure of bifurcation; for any other larger angular momentum one of the solutions belongs to an unstable series and the other to a stable series of figures. Thus, by determining the figure of minimum partial angular momentum, we find the figure of limiting partial stability.

The only solution for which Roche gave a numerical result was that in which the satellite is infinitesimal relatively to the planet. He found that the nearest possible infinitesimal satellite (which is also in this case the satellite of limiting partial stability) has a radius vector equal to 2·44 radii of its spherical planet. He showed the satellite to have an ellipsoidal figure, and stated that its axes were proportional to the numbers 1000, 496, 469. In the paper the problem is solved by more accurate methods than those used by Roche, and it is proved that the radius vector is 2·4553, and that the axes of the ellipsoid are proportional to 10,000, 5114, 4827. The closeness with which his numbers agree with these shows that he must have used his graphical constructions with great care.

For satellites of finite mass the satellite is no longer ellipsoidal, and it becomes necessary to consider the deformation by various inequalities, which may be expressed by means of ellipsoidal harmonic functions

The general effect for Roche's satellites of finite mass in limiting partial stability is that the ellipsoidal form is very nearly correct over most of the periphery of the satellite, but at the extremity facing the planet there is a tendency to push forth a protrusion towards the planet. In the stable series of figures up to limiting stability this protrusion is of no great magnitude, but in the unstable series it would become strongly marked. When the unstable figure becomes much elongated, we find that it finally overlaps the planet, but before this takes place the approximation has become very imperfect.

Turning now to the case of complete secular stability, where the tidal friction in each mass is taken into account, we find that for an infinitely small satellite limiting stability occurs when the two masses are infinitely far apart. It is clear that this must be the case, because a rotating liquid planet will continue to repel its satellite so long as it has any rotational momentum to transfer to orbital momentum through the intervention of tidal friction. Thus an infinitesimal satellite will be repelled to infinity before it reaches the configuration of secular stability. As the mass of the satellite increases, the radius vector of limiting stability decreases with great rapidity, and for two equal masses, each constrainedly spherical, the configuration is reached when the radius vector is 2·19 times the radius of either body.

When we pass to the case where each liquid mass is a figure of equilibrium, the radius vector for limiting stability is still infinite for the infinitely small satellite, and diminishes rapidly for increasing mass of the satellite. When the two masses are equal the radius vector of limiting stability is 2·638 times the radius of a sphere whose mass is equal to the sum of the masses of the two bodies. This radius vector is considerably greater than that found in the case of the two spheres, for the 2·19 radii of either sphere, when expressed in the same unit, is only 1·74. Thus the deformations of the two masses forbid them to approach with stability so near as when they were constrainedly spherical.

In all these cases of true secular stability, instability supervenes through tidal friction, and not, as in the case of Roche's problem, through the deformation of figure.

When Poincaré announced that there was a figure of equilibrium of a single mass of liquid shaped something like a pear, he also conjectured that the constriction between the stalk and the middle of the pear would become developed until it was a thin neck; and yet further that the neck might break and the two masses become detached. The present revision of Roche's work was undertaken in the hope that it would throw some light on the pear-shaped figure in the advanced stage of development.

As a preliminary to greater exactness, the equilibrium is investigated of two masses of liquid each constrainedly spherical, joined by a weightless pipe. Through such a pipe liquid can pass from one mass to the other, and it will continue to do so until, for given radius vector, the masses of the two spheres bear some definite ratio to one another. In other words, two spherical masses of given ratio can be started to revolve about one another in a circular orbit, without relative motion, at such a distance that liquid will not pass through a pipe from one to the other.

The condition for equilibrium is found to be expressible in the form of a cubic equation in the radius vector, with coefficients which are functions of the ratio of the masses. Only one of the three roots of the cubic has a physical meaning, and in all cases the two masses are found to be very close together; but the system can never possess secular stability.

When the masses are no longer constrainedly spherical the equation of condition for equilibrium, when junction is effected by a weightless pipe, becomes very complicated and can only be expressed approximately. It appears that in all cases, even of Roche's ellipsoids in limiting stability, the masses are much too far apart to admit of junction by a pipe; but when we consider the unstable series of much elongated ellipsoids, it seems that such junction is possible, although the approximation is too imperfect

to enable us to draw the figure with any approach to accuracy. If two ellipsoids are unstable when moving detached from one another, junction by a pipe cannot possibly make them stable. This then points to the conclusion that the pear-shaped figure is unstable when so far developed as to be better described as two bulbs joined by a thin neck.

Mr. Jeans has considered the equilibrium and stability of infinite rotating cylinders of liquid.* This is the two-dimensional analogue of the three-dimensional problem. He found solutions perfectly analogous to Maclaurin's and Jacobi's ellipsoids and to the pear-shaped figure, and he was able to follow the development of the cylinder of pear-shaped section until the neck joining the two parts had become quite thin. The analysis, besides, points to the rupture of the neck, although the method fails to afford the actual shapes and dimensions in this last stage of development. He is able to prove conclusively that the cylinder of pear-shaped section is stable, and it is important to note that he finds no evidence of any break in the stability up to the division of the cylinder into two parts.

The stability of Maclaurin's and of the shorter Jacobian ellipsoids is well established, and I imagined that I had proved that the pear-shaped figure with incipient furrowing was also stable. But M. Liapounoff† now states that he is able to prove the pear-shaped figure to be unstable from the beginning. For the present at least I still think it is stable, and this belief receives powerful support from Mr. Jeans' researches.

But there is another difficulty raised by the present paper. I had fully expected to obtain an approximation to a stable figure consisting of two bulbs joined by a thin neck, but although the present work indicates the existence of such a figure, it seems conclusive against its stability. If then Mr. Jeans is right in believing in the stable transition from the cylinder of pear-shaped section to two detached cylinders, and if I am now correct, the two problems must part company at some undetermined stage. M. Liapounoff will no doubt contend that it is at the beginning of the pear-shaped series of figures, but for the present I should dissent from that view.

One question remains: If the present conclusions are right, do they entirely destroy the applicability of this group of ideas to the explanation of the birth of satellites or of double stars? I think not, for we see how a tendency to fission arises, and it is not impossible that a period of turbulence may naturally supervene in the process of separation. Finally, as Mr. Jeans points out, heterogeneity introduces new and important differences in the conditions.

* 'Phil. Trans.,' A, vol. 200, pp. 67—104.

† 'Acad. Imp. des Sci. de St. Pétersbourg,' vol. 17, No. 3, 1905.

On the Coefficient of Viscous Traction and its Relation to that of Viscosity.

By Fred. T. Trouton, F.R.S.

(Received February 12,—Read February 22, 1906.)

When experiments are made on the viscous flow of pitch and other substances of similar character, in the form of rods or cylinders, by the torsional method,* it is found that the rate of turning under torsion of these rods is not strictly proportional to the driving couple. Thus the rate of flow of the material under shearing stress cannot be in simple proportion to stress. If it is wished to investigate the exact law connecting the rate of flow with the shearing forces, by means of the torsional method, a complication is at once met with, arising from the fact that the rate of flow in a twisting rod is not of the same value everywhere, but necessarily varies from nothing at the centre to a maximum at the surface of the rod.

With the view of developing a more suitable way of investigating the phenomenon, trials were made with different methods of observing the flow of such bodies, under conditions in which the said objection does not apply. The results obtained in these ways exhibit the same departures from linearity as was suggested by the results obtained by the method of torsion.

The types of flow examined were: (1) the flow produced in a rod or cylinder of the material when under traction; (2) when under axial compression; (3) the flow of a freely descending stream of the material; (4) the rate of bending of a horizontal rod or beam of the material under its own weight when supported only at the ends.

The latter method, however, suffers from the same defect as the torsional method, namely, from giving an integral value through a range from zero up to a certain limit.

Traction Experiments.

Rods or bars of the material under examination were suspended from one end, while to the other weights were attached. The rod was thus subjected to a force which continuously drew it out. The rate of elongation per centimetre length of the rod could then be determined.

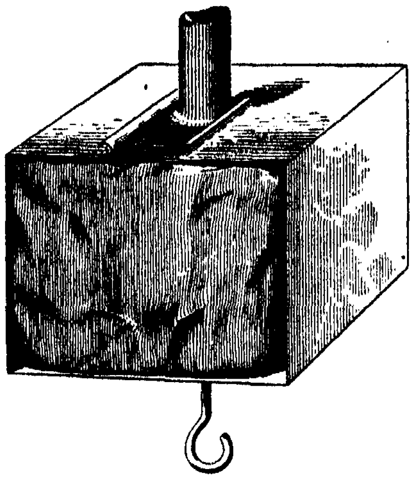
The rods for these experiments were prepared by the method described in the paper already referred to. For the purpose of applying the tractional forces the rod was thickened at each end and worked into an almost cubical block, as shown in fig. 1, so as to fit a metallic box or receptacle open to one

* 'Phil. Mag.,' vol. 19, p. 347, 1904.

side, by which the rod could be hung from a support at the top and a weight attached at the lower end. The opening in the box for the rod was slotted out to one side so as to admit the rod being slid into its place.

In estimating the stress in the rod we must add to the weight attached to the lower hook the weight of the box and of the pitch below the lower point of observation on the rod. The weight of the rod under observation itself produces an increase in the stress from the lower end upwards, its amount half way up is obviously half that of the rod between the lower and upper mark. For corresponding points above and below the middle the stress is in excess and defect of the mean value by equal amounts, so that, assuming linearity for the flow, the correction will be half the weight of the portion of the rod in the observation. The rate of elongation of the rod was observed by means of a cathetometer.

FIG. 1.



In observations with certain materials such as shoemaker's wax the plan was adopted of experimenting under a liquid of the same density as the material itself for the purpose of eliminating the stress due to its own weight, which was much too great to admit of accurate observations being otherwise made on the rate of flow.

The lower end of the rod, in such cases, was made fast while the tractive weight was applied at the top by means of a cord and pulley. The rods were thus drawn upwards. This was done in order to be able the more easily to surround the rod with a liquid, having the same density as its material, and thus to eliminate the action of the rod's own weight. For holding the liquid a wide vertical tube was fitted to the apparatus and surrounded the rod under examination.

Solutions of salt in water were used for the liquids.

Results of Traction Experiments.—The results obtained with pitch and other substances showed that the time rate of elongation per centimetre of a rod under tension is approximately proportional to the force of traction per square centimetre cross section or $\frac{F}{A}\Big/\frac{dv}{dx} = \lambda$, where F is the force applied, A the area of the cross section, v the velocity at any point x on the rod, and λ

a constant for any given material. This we shall term the coefficient of *viscous traction* of the material.

The values found for λ for a few of the substances experimented with are given to show the order of the coefficient.

For an ordinary variety of pitch at 15° C.	$4{\cdot}3 \times 10^{10}$
For the same with a slight admixture of tar	$6{\cdot}7 \times 10^{9}$
For shoemaker's wax at 15° C.	$5{\cdot}9 \times 10^{6}$

Initial Rate of Flow.—The observations made on the rate of flow of rods of these substances show that it is faster immediately after the application of the force than afterwards.

As an example, the following table is given, in which the elongation is in arbitrary units and the time is in minutes and seconds to the nearest five seconds. These observations are plotted in fig. 2.

FIG. 2.

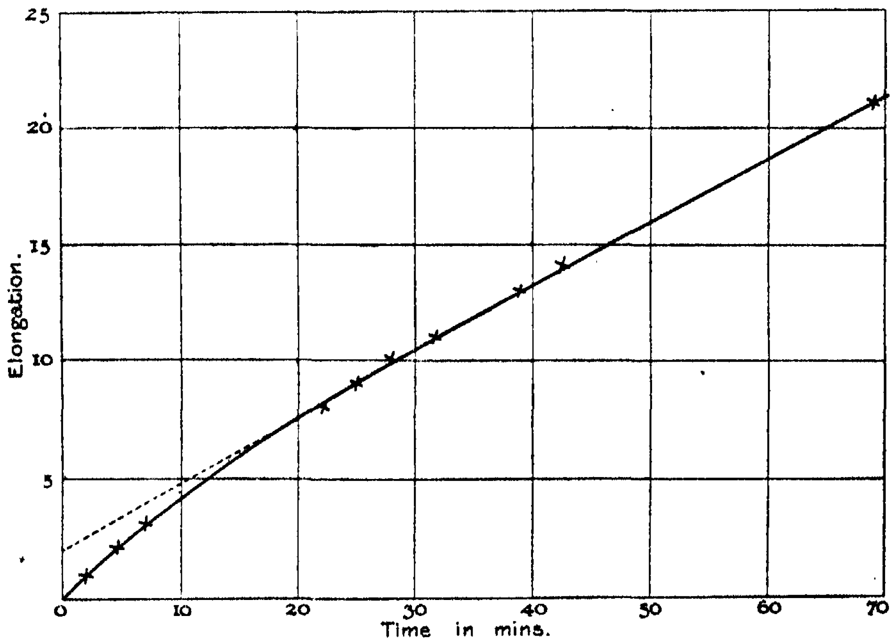


Table I.

δl ...	0.	1.	2.	3.	8.	9.	10.	11.	13.	14.	21.
t......	0	2′ 0″	4′ 25″	6′ 50″	22′ 0″	24′ 55″	28′ 0″	31′ 35″	39′ 0″	42′ 40″	69′ 0″

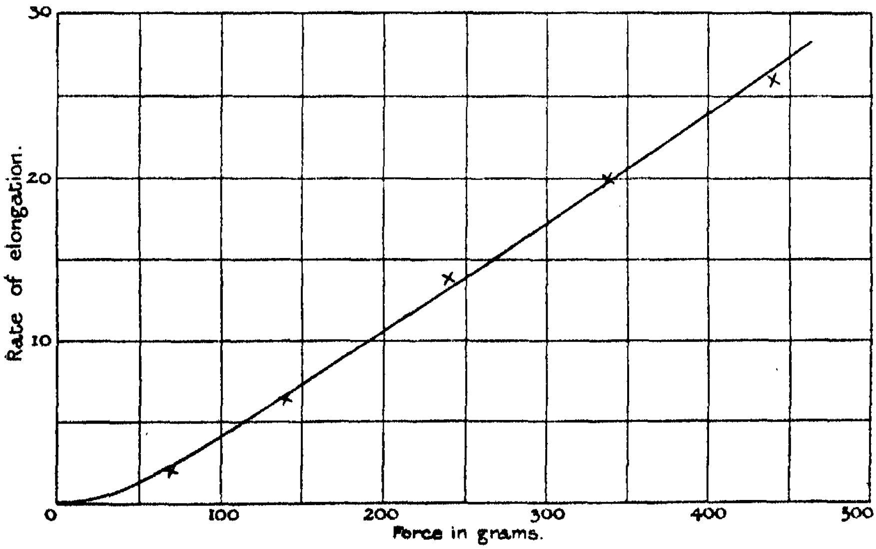
It will be seen that the initial rate of flow is faster than the final rate. This is similar to what was observed in the case of the viscous flow of rods under torsion.* It was also noticed that there was a slow partial movement towards recovery on removal of the force of traction, which gradually fell to zero with time, just as had been previously observed in the case of torsional forces.

Departure from Linearity with Variation of Tractive Force.—Experiments made with different values of the tractive force show that the rate of flow is not strictly proportional to the force. The results of determinations made with a variety of pitch given in Table II and shown in fig. 3 are typical.

Table II.

Force.	70.	140.	240.	340.	440.
Rate of elongation ...	2·0	6·4	14	20	26

Fig. 3.



The ordinates represent rate of elongation, while the abscissæ represent the force applied to the rod in grammes. It will be seen from the curve that except near the origin the law is linear. For forces above a certain value the rate of flow may be expressed as $T - T_0 = \lambda dv/dx$. The rate of elongation

* 'Phil. Mag.,' vol. 19, p. 347, 1904.

taken for the curve was in every case that obtaining after the initial stage had been passed.

The Paths of Flow of the Particles in a Drawn-out Rod.—The mode of flow of the particles of a rod while being drawn out is of interest. With the view of experimentally ascertaining if the particles lying in a cross section moved out symmetrically on thinning taking place, that is to say, if half of the particles, scattered uniformly throughout a cross section, are left relatively behind, rods were prepared of materials having approximately the same coefficient of viscous traction, but of different colours. Two different coloured rods were united end to end and then drawn out, the surface of demarcation being carefully watched.

Rods of different coloured glass were tried for this purpose and, provided they are approximately of the same fusibility, answered well. Compound rods thus made were warmed and drawn out and the surface where they joined observed. This always remained a plane; though sometimes it did not remain a cross section. No tendency was noticed for the central portion to flow at a different rate to the peripheral parts. The flow shows itself to be quite symmetrical along the axis, as one would naturally expect. Similar results were obtained with shoemaker's wax. In this case the coloration of one end was effected by the addition of a small quantity of vermilion. On slicing the drawn-out rod, the line of demarcation could be seen.

Axial Compression of Cylinders.

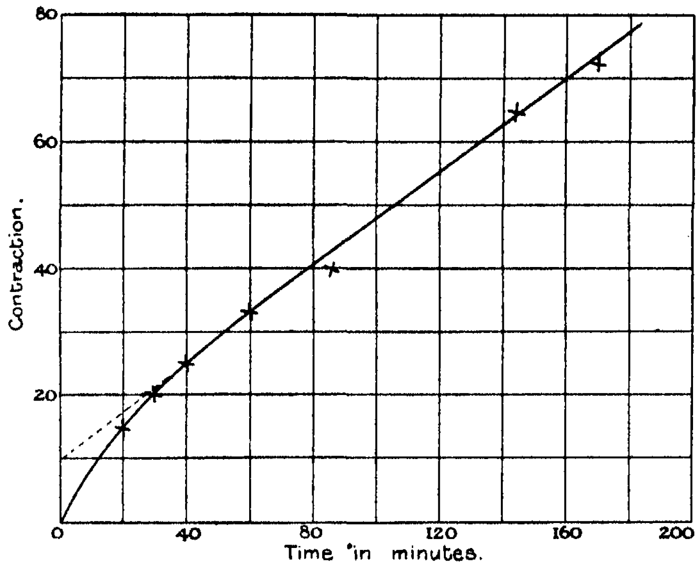
Experiments on the rate of axial contraction of cylinders under axial compression gave results corresponding with those obtained for the traction of rods, as detailed above. That is to say:—(1) The rate of contraction on first application if the stress is a little faster than that finally reached (see fig. 4); (2) there is a slow movement towards recovery on removal of the stress; and (3) the final rate of flow increases at a uniform rate with the increase in the applied stress when the latter is above a certain limiting value.

In the case of traction, the length of the rod or cylinder can be large compared with its diameter, in most cases it was between 20 and 30 times the diameter, but for compression on account of buckling, it is well for the length to be not more than about three times the diameter of the cylinder.

The stress was applied by placing weights on a plate which just covered the top of the cylinder, which stood vertically. The rate of depression of the cylinder was observed by means of a cathetometer. The coefficients obtained from experiments on compression made in this way were found to be about the same in magnitude as those obtained from traction. For instance, with

a certain specimen of a rather soft pitch, the compressional coefficient was found to be $7{\cdot}6 \times 10^9$, while the coefficient for traction of the same material was found to be $6{\cdot}7 \times 10^9$.

Fig. 4.



Flow of a Stream Descending under its own Weight.

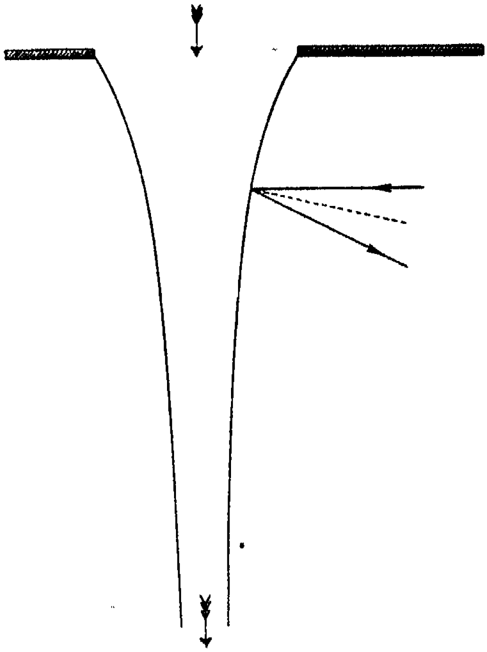
The stream of the material under examination was obtained by allowing it to flow through a circular hole in the bottom of a wide tin vessel. After a steady state is reached, the outline of the stream is of the character shown in fig. 5. The stream gradually tapers down to a very fine thread, which breaks off intermittently from its lower end.

At any point distant x from the top let v be the downward velocity of the material, then the time-rate of elongation per centimetre of the material at this point is dv/dx. The tractive force is due to the mass M of the column below this point. Let the cross section be πy^2, we have then, provided the velocity is small, $Mg = \lambda \pi y^2 dv/dx$. Thus we may write $\lambda = -\dfrac{Mg}{2p}\dfrac{y}{dy/dx}$, where p is the volume of material ($\pi y^2 v$) supplied per second, and dy/dx the slope of the tangent to the surface at the point of the column where $2y$ is the diameter.

Tangent Method of Determining λ.—An optical method can be adopted for determining the slope of the surface of the falling column at a definite point. A beam of light from a horizontal slit and lens is allowed to fall on the

column and is reflected into a telescope which is pointed upwards to receive it. The tangent of half the angle between the incident and reflected beams is evidently the slope of the surface neglecting curvature.

FIG. 5.



The value of M can be got by cutting off the column at the point in question, collecting and weighing. The value of M varies, it is true, according as the column breaks off at its lower end or otherwise, but only to a negligible extent.

The value found for the coefficient of viscous traction by this method for a mixture of pitch and tar in the ratio of 7 to 1 was $1{\cdot}1 \times 10^8$; and for a mixture of the same materials in the ratio of 3 to 1 about $9{\cdot}3 \times 10^6$. These are about the same values as obtained by the other methods,

Investigation of Shape of a Stream Descending under its own Weight.—To determine the shape of the column we have the following considerations. At any point we have seen that the force of traction is

$$\mathrm{F} = \lambda \mathrm{A} dv/dx, \tag{1}$$

where A is the area of the cross section, so that $v\mathrm{A} = \text{const}$. Also

$$\frac{d\mathrm{F}}{dx} + g\rho\mathrm{A} + \mathrm{A}\rho\left(\frac{dv}{dt} + v\frac{dv}{dx}\right) = 0, \tag{2}$$

when ρ is the density of the material.

If the rate of fall is small, the acceleration term may be neglected, and we get

$$\lambda \frac{d}{dx}\left(\mathrm{A}\frac{dv}{dx}\right) = -g\rho\mathrm{A}.$$

Substitute for v from the relation $v\mathrm{A}\rho = \mathrm{M}_1$, where M_1 is the mass of material falling per second; and substitute πy^2 for A. Then, after differentiating and arranging, we have

$$\frac{1}{y^3}\frac{d^2y}{dx^2} - \frac{1}{y^4}\left(\frac{dy}{dx}\right)^2 - 1/\mathrm{K}^2 = 0,$$

where $\mathrm{K}^2 = 2\lambda\mathrm{M}_1/g\rho^2\pi$. The general solution of this equation is $y = b/\sinh\frac{bx}{\mathrm{K}}$. When b is very small it represents a long filament, such as in the present case. The limiting solution when $b = 0$ is $xy = \mathrm{K}$. This last expression was found, as described below, to fit experimental data with sufficient accuracy.

In order to experimentally examine the question, mixtures of pitch and tar were made sufficiently thick or glutinous for the flow to be slow enough to enable the acceleration term to be neglected. The curvature assumed by the descending column was experimentally determined by observing the diameter of the column at various heights. This was done in some cases by means of a cathetometer, in others by casting the shadow of the column from a distant source of light on a long vertical sheet of paper placed close to the column, and then tracing out the shadow with pencil.

The cathetometer telescope had a scale in the eye-piece, with which the horizontal breadth of the column at the different heights was observed according as the telescope was raised to various positions along the column. From these readings, in conjunction with the height readings, the curve made by the pitch in falling could be plotted. It was then found possible to fit an equilateral hyperbola to it.

The following table (p. 434) gives the results obtained with rather a thick mixture. The first column gives the height in centimetres; the second one half the observed diameter; the third column gives the calculated value of the radius derived from the formula $y(x+m) = \mathrm{K}$.

The values of the constants used were $\mathrm{K} = 1{\cdot}85$ and $m = 1{\cdot}8$. This last is the height above the bottom of the vessel at which the horizontal asymptote is situated.

The divisions of the scale of the eye-piece corresponded to 0·03 cm. This was subdivided by eye, so that the agreement is quite within the limits to be expected. There was some difficulty at times in reading the diameter, especially towards the lower end, as the hanging column would sometimes sway slightly about, even though it was placed inside a tall glass case with

front to shelter it from draughts. This was due in large measure to the effect of the end breaking off.

Table III.

x.	y obs.	y calc.	Diff.	x.	y obs.	y calc.	Diff.
0	1·035	1·027	−0·008	8	0·190	0·188	−0·002
0·5	0·790	0·804	+0·014	9	0·165	0·171	+0·006
1	0·635	0·660	+0·025	10	0·155	0·156	+0·001
1·5	0·550	0·560	+0·010	12	0·135	0·134	−0·001
2	0·475	0·460	−0·015	15	0·110	0·104	−0·006
2·5	0·430	0·430	0	20	0·090	0·089	−0·001
3	0·370	0·385	+0·015	30	0·065	0·058	−0·007
3·5	0·340	0·349	+0·009	40	0·045	0·044	−0·001
4	0·315	0·318	+0·003	50	0·030	0·035	+0·005
4·5	0·290	0·293	+0·003	66·5	0·020	0·027	+0·007
5	0·255	0·271	+0·016	88	0·020	0·020	0
6	0·225	0·237	+0·012	92·5	0·015	0·019	+0·004
7	0·200	0·210	+0·010				

The Value of λ found by Falling Column Method.—From the value of the constant in $xy = K$ the coefficient of viscous traction may be calculated. Thus

$$\lambda = \rho^2 g\pi K^2/2M_1.$$

Taking as found above $K = 1{\cdot}85$ $\rho = 1{\cdot}32$ and $M_1 = 0{\cdot}0000950$, we get $\lambda = 9{\cdot}6 \times 10^7$. This was at a mean temperature of 16° C. A rod made from the same mixture of tar and pitch gave for λ by the traction method the value $\lambda = 7{\cdot}8 \times 10^7$ at 17°·5 C. Another rod from the same mixture gave 13×10^7 at 14° C. The agreement will be considered sufficiently good to confirm the theory when the character of the material is remembered. Unfortunately, though very convenient for making materials of any desired viscosity, these mixtures suffer from the disadvantage that when they are heated for the manufacture of the test rods they lose some of their more volatile constituents, becoming more viscous in consequence.

Modification Introduced by Inertia.—The modification from the hyperbolic form, which the falling stream of material undergoes when it is not so viscous as to render the inertia term negligible, may be appreciated by noting that when the shape is hyperbolic the acceleration varies as the third power of the height fallen, so that though at first the inertia term may be quite negligible, it may at lower points become important and sensibly reduce the traction effect. In this way the contour at lower points assumes less of the hyperbolic and approximates more towards the cylindrical shape.

In the limit where viscosity is negligible the shape may be taken as given approximately by $xy^4 = K$, provided we ignore the tendency to break up into

drops due to surface tension. Comparing the two cases, say for points x_1 and x_2, where $x_2 = 2x_1$, we have for the non-inertia case $y_2 = 0{\cdot}5y_1$, for the non-viscous case $y_2 = 0{\cdot}84y_1$. Where both causes act, some intermediate shape will be assumed by the falling material. Descending columns of viscous liquids, such as the familiar one of honey falling from a spoon, form instances of this.

Sagging of a Horizontal Beam.

If a rod of pitch is laid across between two horizontal supports it will be found to continuously sag downwards. The rate at which this occurs varies with the consistency of the material.

To find how the rate of sagging depends on the coefficient of viscous traction, we can resolve the stresses in the material at any cross section after the manner usual in the cases of stressed beams. This gives compressional forces above and tractive force below a certain point in any cross section. Taking this as being at the central horizontal line of the cross section, we have for the moment of the force about this line $M = \lambda \int \left(\frac{dv}{dx}\right)_y y \,.\, b \,.\, dy$, where $\left(\frac{dv}{dx}\right)_y$ is the rate per unit length of the elongation, or the contraction as the case may be, at any point situated at distances y from the central line, and where b is the breadth there.

This value for M may be approximately expressed in terms of the rate at which a plane in the material at the point rotates at the moment when it is at right angles to the axis; thus $\left(\frac{dv}{dx}\right) = y\,\frac{d\omega}{dx}$, where ω is this rate of rotation at any cross section, so that $M = \lambda I\,\frac{d\omega}{dx}$.

Now u, the rate of sagging at the centre, is given by $u = \Sigma_0^{\frac{1}{2}L} x\delta\omega$; and, recollecting that $M = \frac{1}{2}gm\left(x - \frac{x^2}{L}\right)$, where m is the mass of the rod between the supports and L its length, we get, after arranging and integrating,

$$u = \frac{5}{384}\,\frac{gmL^3}{\lambda I},$$

where I is the moment of inertia of the cross section of the rod.

One of the methods employed to test this formula was to compare the rate of sagging of rods of different lengths, all other quantities involved being unaltered. A rod of pitch of circular cross section was laid between two supports which could be placed at various distances apart, and the time of sagging through the same distance in each case observed. In all cases the

initial rate of sagging was not included in the measurements. To allow this to be done the rod was made with a camber and the observations only begun just before attaining the straight position.

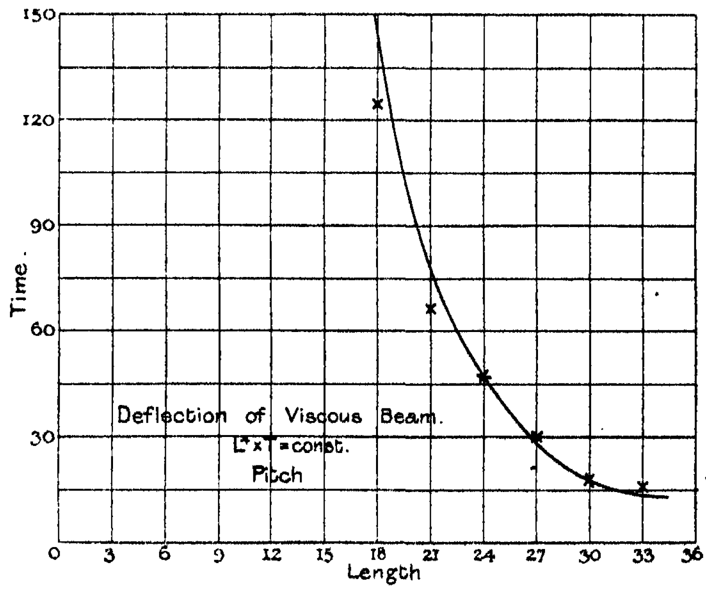
It will be seen on comparing rods of different length, if T represents the time taken, at any given span L, to sag through the standard distance, that TL^4 = const. The following table gives in the first column the temperature at the time of the experiment, in the second column the span, in the third the time taken to fall through a standard distance, and in the fourth the value found for this constant in each case.

Table IV.

°C.	L.	T.	TL^4.
15°	33	14·6	$1·7 \times 10^7$
15	30	18·5	1·5
15	27	30·4	1·6
15	24	47·0	1·6
18	21	66·2	1·3
18	18	125·0	1·3

The last two experiments were made on a different day from the others, and were made at a slightly higher temperature. This may in part account for the smaller value found for the constant. The curve obtained by plotting L against T is shown in fig. 6.

Fig. 6.



The Initial Rate of Sagging.—As in other cases previously dealt with, so in the case of sagging beams, the initial rate of flow is greater than subsequently. The following table, obtained from experiments with a certain variety of pitch, illustrates this. In the first column are given the distances fallen from zero by the central point of a rod or beam of pitch. In the second column the time taken to each point. The curve obtained by plotting these is shown in fig. 7.

FIG. 7.

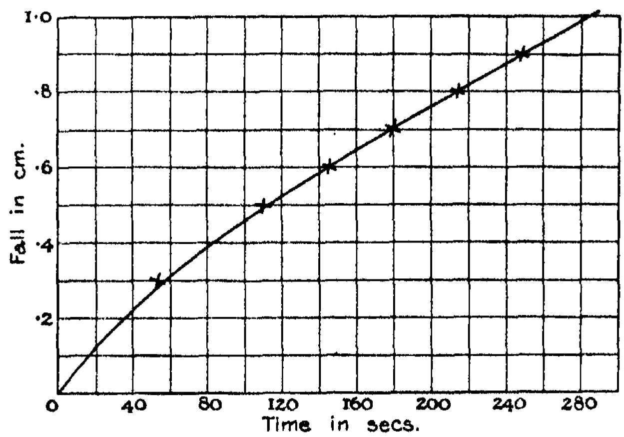


Table V.

h.........	0·0.	0·3.	0·5.	0·6.	0·7	0·8.	0·9.	1·0.
t.........	0	55	110	145	180	215	250	290

The dimensions of this rod, which was circular, were as follows: length between supports $L = 25$, mean radius $R = 0{\cdot}47$, mass between supports $m = 21{\cdot}7$, final rate of sagging $U = 0{\cdot}00285$ average. This gives, using above formula, $\lambda = 3{\cdot}96 \times 10^{10}$. The same rod gave by the traction method $3{\cdot}90 \times 10^{10}$.

A number of other mixtures of pitch and tar of varying proportions were experimented with. These gave approximately the same value for the coefficient of viscous traction as that found by either the falling column method or by the direct traction method.

Some of the mixtures of pitch and tar were too soft to deal with as a beam in air, so they were experimented with under water or under strong brine. In this way the downward bending moment could be very greatly reduced, or, if desired, could be even changed in sign, when, of course, the

ends of the beam had to be held down. A particular mixture whose density was 1·185 gave the value $\lambda = 3{\cdot}01 \times 10^5$ when operated under water. The value previously found for it by the falling column method was $= 4{\cdot}3 \times 10^5$. The velocity in this case was perhaps too great to neglect the inertia term in the falling column method, and may account for the higher value of the coefficient obtained by it.

Connection between the Coefficient of Viscous Traction and the Coefficient of Viscosity.

It is obvious that there must be an intimate relation between the coefficient of viscous traction and the coefficient of viscosity as ordinarily defined. The tractional force applied to a rod may be resolved, as is usual in questions of elasticity, into two equal shears, which are situated at right angles to each other and at 45° to the direction of traction, along with a uniform force of dilatation. The value of either shearing stress, and also of the dilatation stress, is in each case one-third of that of the tractive stress.

In the first instance on the application of the tractive force, the resolved effects produced corresponding to these resolved stresses will consist of a dilatation and of shearing strain. It can only be to the flow resulting from the latter that the *continued* elongation of the rod is due. Nothing similar can take place in the case of the stress of dilatation, which only can have an initial effect.

The continued application of each shear will produce a corresponding flow, given in each case by $S = \mu\dot{\phi}$, where S is the shearing stress, μ the coefficient of viscosity, and $\dot{\phi}$ the rate of change of direction of any line in the material in the plane of the shear as it passes through the direction normal to the shearing stress. The resulting flow in the direction of the axis is obtained by adding the resolved components of the two flows in that direction; so that resolving the two effects, adding the components, and reducing the axial elongation to that (e) per unit length, we find that $e = \dot{\phi}$.

Since $T = \lambda e$, and $S = \frac{1}{3}$ T, where T is the tractive force per square centimetre, we get $\mu = \frac{1}{3}\lambda$, so that the coefficient of viscosity is equal to one-third of the coefficient of viscous traction.*

* In terms of the more usual analysis of viscous flow, with constant stress-modulus, the argument would take the following form:—Consider a viscous cylinder undergoing elongation at rate e; if its material is but very slightly compressible, it must at the same time undergo contraction at rate $\frac{1}{2}e$ in all transverse directions. If μ represent the viscosity of the material, this rate of elongation implies a longitudinal tension of intensity $2\mu e$, and similarly there is transverse tension of intensity $-2\mu \,.\, \frac{1}{2}e$. These tractions

In order to compare the coefficient of viscous traction with that of viscosity for the same material, two distinct plans were adopted. One was to select a material sufficiently viscous to allow the coefficient of viscosity to be determined by means of the torsion of a rod made of it,* and also which allowed the coefficient of viscous traction to be found by directly drawing out the rod, or by the method of the sagging horizontal beam. The second plan was to select a material sufficiently fluid to admit of the coefficient of viscosity being determined by the rate of flow through a tube under a pressure head, while at the same time not so fluid but that the coefficient of viscous traction could be observed by the method of the sagging beam or by the method of the column descending under its own weight.

The following are the results obtained for the value of λ and μ in the case of several materials of wide range in the value of the constants. It will be seen that the value of λ is, generally speaking, in fair agreement with three times the value of μ, the viscosity.

A variety of pitch which gave by the traction method $\lambda = 4{\cdot}3 \times 10^{10}$ was found by the torsion method to have a viscosity $\mu = 1{\cdot}4 \times 10^{10}$. Another variety of pitch gave $\lambda = 3{\cdot}6 \times 10^{10}$ by the traction method and $\lambda = 3{\cdot}3 \times 10^{10}$ by the sagging beam method, while the viscosity was found to be $= 1{\cdot}0 \times 10^{10}$ by the torsion method.

A material made by adding a little tar to pitch gave by the traction method $\lambda = 12{\cdot}9 \times 10^{9}$ and $\mu = 4{\cdot}2 \times 10^{9}$ by the torsion method. A similar material containing a little more tar gave $\lambda = 6{\cdot}7 \times 10^{9}$ by the traction method and $= 2{\cdot}2 \times 10^{9}$ by the torsion method.

A specimen of shoemaker's wax gave $\lambda = 5{\cdot}9 \times 10^{6}$ by the traction method and $\mu = 2{\cdot}0 \times 10^{6}$ by the torsion method.

For making a comparison by the tube method a mixture of pitch and tar of about three to one was used. This passed sufficiently freely through a tube to enable the coefficient of viscosity to be determined. This was found to be $\mu = 2{\cdot}6 \times 10^{5}$, while the coefficient of viscous traction was found by the sagging beam method to be $= 7{\cdot}6 \times 10^{5}$. Another mixture of somewhat similar proportions, but better filtered, gave $\mu = 2{\cdot}8 \times 10^{5}$ by the tube method and $\lambda = 9{\cdot}3 \times 10^{5}$ by the descending column method.

acting on the surfaces of the cylinder amount in all to a uniform hydrostatic pressure μe, together with a longitudinal tension of intensity $3\mu e$. Of these the pressure is entirely neutralised by the reaction arising from the slight compression of the materials which it produces; while the longitudinal tension, having an intensity-coefficient 3μ, alone remains to operate in other ways, as in the text.

* 'Phil. Mag.,' vol. 19, p. 347, 1904.

These results are collected in Table VI, where it will be seen that the coefficient of viscous traction λ is roughly three times that of viscosity.

Table VI.

λ.	μ.	λ/μ.	λ.	μ.	λ/μ.
$4 \cdot 3 \times 10^{10}$	$1 \cdot 4 \times 10^{10}$	3·07	$6 \cdot 7 \times 10^{9}$	$2 \cdot 2 \times 10^{9}$	3·04
$3 \cdot 6 \times 10^{10}$	$1 \cdot 0 \times 10^{10}$	3·60	$5 \cdot 9 \times 10^{6}$	$2 \cdot 0 \times 10^{6}$	2·95
$3 \cdot 3 \times 10^{10}$		3·30	$9 \cdot 3 \times 10^{5}$	$2 \cdot 8 \times 10^{5}$	3·25
$12 \cdot 9 \times 10^{9}$	$4 \cdot 2 \times 10^{9}$	3·07	$7 \cdot 6 \times 10^{5}$	$2 \cdot 6 \times 10^{5}$	2·91

The Vertical Temperature Gradients on the West Coast of Scotland and at Oxshott, Surrey.

By W. H. Dines, F.R.S.

(Received November 10,—Read December 7, 1905.)

In a paper by Dr. Shaw and the author read before the Royal Society on May 14, 1903,* an account of an investigation into the conditions of the upper air over the sea in the neighbourhood of Crinan, on the West Coast of Scotland, was given. Since that time two fresh series of observations in the same locality have been obtained, the results of which are now submitted. In each case observations of temperature and humidity were made by self-recording instruments sent up by means of one or more kites, which were flown from the deck of a steam vessel.

Expenses.

The expense has been met by a grant of £200 made by the Government Grant Committee, a grant of £50 made by the British Association at the Southport Meeting, and of £40 at the Cambridge Meeting; and also by an anonymous contribution of £25 by a Fellow of the Royal Meteorological Society. These grants have not been used entirely for the observations at Crinan, but have afforded the means of carrying on experimental work at Oxshott; by them, too, apparatus for a separate investigation carried out by Mr. G. Simpson on the North Sea has been provided.† For the observations at Crinan in 1903 a tug was hired, and the Lords Commissioners of the

* 'Phil. Trans.,' A, vol. 202, pp. 123—141.

† 'Met. Soc. Quart. Journ.,' vol. 32, No. 137, pp. 15—25.

Admiralty, at the request of the Royal Society, kindly provided a very convenient vessel, H.M.S. "Seahorse," for six weeks, commencing on June 19, 1904. The Meteorological Council lent the necessary instruments, and bore the expense of maintaining a base station during both summers. They also greatly assisted the work by sending a daily telegram with a forecast of the weather, and a statement of the magnitude of the barometric gradient.

Material Collected.

The results obtained consist of 16 fairly satisfactory traces at Crinan in August, 1903, of 29 traces at Crinan in June and July, 1904, and of 75 traces at Oxshott. The average height for the first set (Crinan, 1903) is 4500 feet, the second set (Crinan, 1904) give 5340 feet. The averages for Oxshott are 3300 feet for 1904 and 5200 feet for 1905. A meteorograph kindly supplied me by M. Teisserenc de Bort was used in 1903; since the beginning of 1904 a new form of meteorograph, which is described in the Quarterly Journal of the Royal Meteorological Society,* has been used.

With the exception of the months of July, August, and September, 1904, when there has been sufficient wind ascents have been made at Oxshott on the days appointed by the International Aeronautical Commission, these days being, as a rule, the first Thursday in each month. The other ascents at Oxshott have been made rather with a view of testing and, where possible, improving the apparatus than of obtaining definite information. Owing to the inconvenient situation of the place, which is close to many thickly-populated suburbs, the rule was made of never using more than one kite. This rule was rigidly adhered to until the commencement of the present year, but since it was found that increased experience and an improvement of the apparatus had led to a great diminution in the number of mishaps, the rule was broken through, and the number of kites extended to two. It is still not considered desirable to use more than two, or to let out more than from 10,000 to 12,000 feet of wire.

It may be of interest to others engaged in similar work if I state that out of the 75 ascents at Oxshott, on five occasions the kite and meteorograph have broken away or fallen to the ground; on one occasion this was due to the breaking of the wire near the kite under a pull of 200 lbs.; on another occasion to the breaking of the front stick of a kite under a pull of 230 lbs.; once to a careless fastening at the end of the wire, and twice to some unknown cause. The loss of material has been one kite broken beyond repair, and about 2000 feet of wire. The meteorograph has never been lost or damaged, since being secured in the middle of the kite it is well protected.

* Vol. 26, No. 135, pp. 217—227.

On the three occasions when the kite has been detached from the wire it has fallen at a horizontal distance of about six times its vertical height, and the wire has been wound in undamaged almost to the end. The average ratio of the vertical height to the length of wire employed has been a little over 6/10.

Observations at Crinan in 1903.

The results obtained in the summer of 1903 have been published in the Journal of the Royal Meteorological Society.* This set of observations were not so numerous or so consecutive as might be wished. The steam tug employed was in many ways unsuitable, and the weather rendered kite flying difficult as well as unpleasant. The observer at Malin Head, the reporting station nearest to Crinan, reported a gale to the Meteorological Office on 20 occasions during August, 1903, and the captain of the tug often feared to take out his vessel, which was old, on account of the weather. The following is a brief summary of the results:—

The average decrease of temperature for the first 500 metres was $2^\circ{\cdot}2$ C., for the second 500 metres $3^\circ{\cdot}4$, and for the third $3^\circ{\cdot}6$, the total decrease for 1500 metres being $9^\circ{\cdot}2$, or at the rate of $0^\circ{\cdot}6$ C. per 100 metres.

The winds were nearly all between south-west and north-west. The temperature observed at the kite when at the level of Ben Nevis was always above the temperature of the summit at the same time, the differences ranging between $1^\circ{\cdot}6$ C. and $6^\circ{\cdot}1$ C., and the average excess was $3^\circ{\cdot}3$ C. One trace of especial interest was obtained: a thunderstorm broke out without previous warning while a kite was flying at a height of 4400 feet (1300 m.), and a meteorograph was drawn in through the actual region of the electrical disturbance, but the trace showed no peculiarity of any kind.

For convenience of reference Table A, which appeared in part in the previous paper and, excepting the values for Oxshott, has been published in the Journal of the Royal Meteorological Society, is here reproduced.

Observations at Crinan in 1904.

The results obtained from H.M.S. "Seahorse" are exhibited graphically in fig. 1.

The diagram (fig. 1) has been drawn on the same plan as before. The isothermal lines for each degree Centigrade are put in as fully as the observations will permit, and where these have failed the isotherms are connected by dotted lines as a guide to the eye. The letters S, M, D, and VD refer to the humidity, S denoting a value between 100 and 95 per cent., M between 95 and 80, D between 80 and 60, and VD below 60 per cent. No great degree of accuracy is claimed for these values.

* Quart. Journ.,' vol. 25, No. 130, p. 155.

Table A.—Table of Average Temperature Gradients in Degrees Centigrade for 100 Metres for Vertical Columns of different Heights.

	Height of column.					
	500	500 to 1000.	1000 to 1500.	1500 to 2000.	2000 to 2500.	2500 to 3000.
Berlin balloon ascents	...	0·50	...	0·50	...	0·51
Kite ascents, U.S	1·10	0·80	0·71	0·68		
Average gradient for mountains	0·56 C. per 100 metres approximately 1° F. per 300 feet.					
Average gradient for Ben Nevis, July and August	1340 metres, 0·70.					
Adiabatic gradient for saturated air, initial temperature 12° C.	0·52	0·54	...	0·53	...	0·62
Average summer gradient over sea, west coast of Scotland 1902...	0·56	0·56	0·52	0·50	0·48	0·46
1903...	0·44	0·68	0·73			
1904...	0·77	0·53	0·34	0·49	0·51	
Average gradient near London (Oxshott), 1904 and 1905	0·77	0·57	0·37	0·30		

Fig. 2 is taken from the official publications of the Meteorological Office, and shows the movement of the depressions for June and July, 1904.

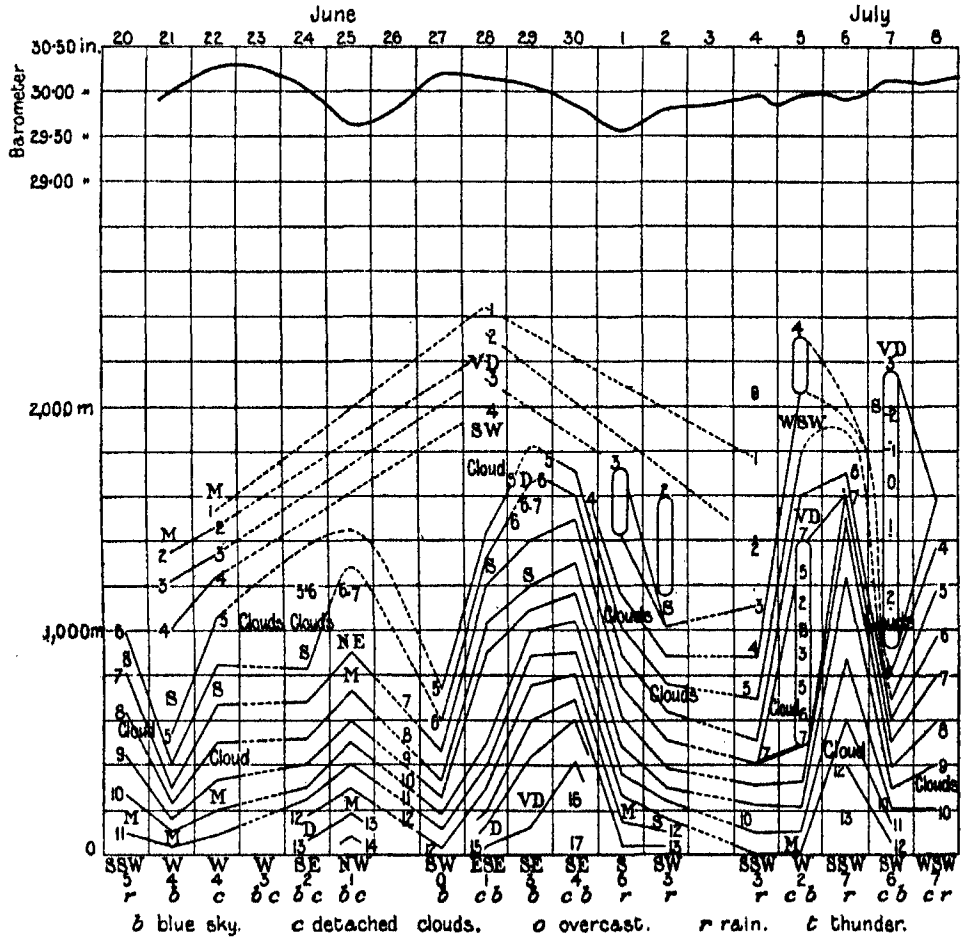
The heights at which clouds appeared are somewhat uncertain. In determining cloud heights by an ascent of a kite there are three possible contingencies; the kite may become indistinct but not entirely vanish during the passage of an opaque cloud, and in this case the height of the kite certainly agrees with that of the lower surface of the cloud. This case is unusual. In the second case the kite disappears into or emerges from a thick cloud sheet; here again the height of the lower surface is perfectly determinate, but it seldom happens that the disappearance and the emergence, perhaps an hour later, occur at anything like the same height. The third case is the most usual, when the kite disappears in or behind an opaque cloud, or becomes indistinct owing to a semitransparent cloud. All that can be said here is that the cloud level is below the height of the kite, and an inspection of the humidity trace often shows that the kite at the time was in very dry air, and therefore not in the cloud at all. If it is a large thick cloud that is in question, the kite need not have been above its upper level, but merely, from the observer's point of view, behind some part of it.

The wind direction, wind force on the Beaufort scale, and weather each day are shown below in the diagram (fig. 1). Where the wind direction at the level of the kite differed much from the surface direction, it has been put in at the corresponding height, and the absence of any letters on the diagram indicates that the surface and upper wind were substantially the same. The

barometer curve shown at the top is copied from a Negretti and Zambra's barograph that was kept at Crinan.

The period under review, June 20 to July 28, 1904, had weather of a somewhat exceptional character for the locality. The prevalent summer wind is certainly west, *i.e.*, between south-west and north-west, but while

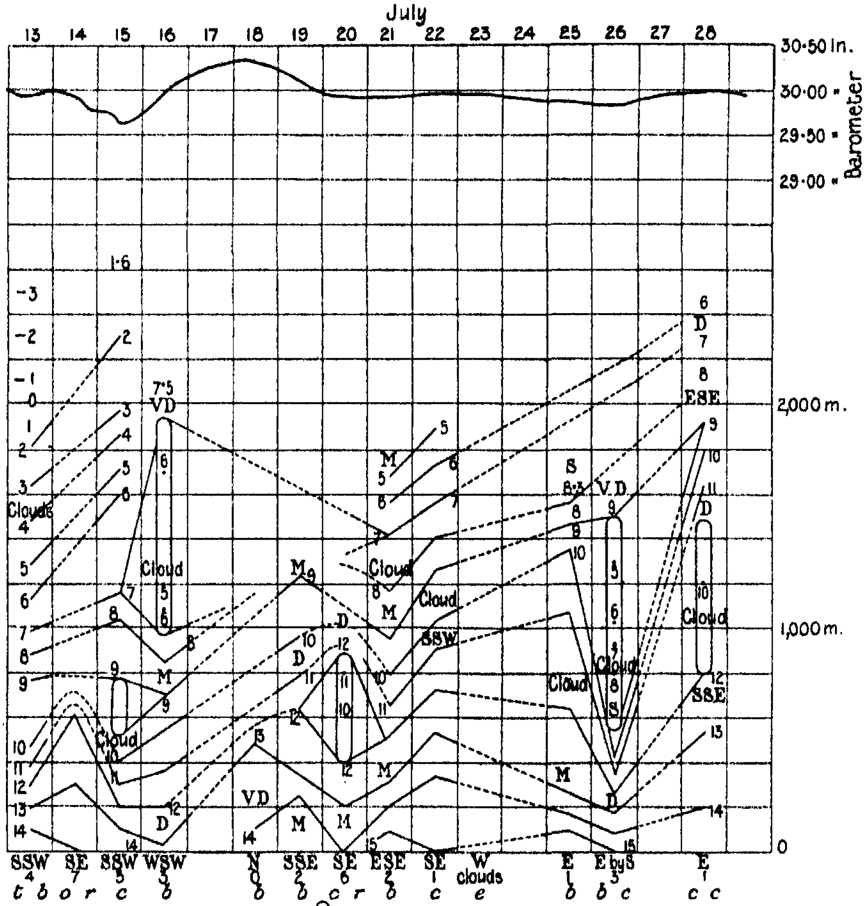
FIG. 1.



the "Seahorse" was at Crinan the wind with few exceptions came from between east and south-west. There were strong winds from some southerly point on several occasions until July 15, but after that date very light winds from the east and south-east were the rule. On three days no ascent could be obtained, and it may be remarked that never before when employing a vessel did we fail to get some sort of daily ascent.

After July 19 the barometer trace becomes almost a straight line, and it was found that the winds failed entirely at the height of a few thousand feet. This was most noticeable on July 20, the only day after the 15th on which there was a good breeze. Although at the surface the force was 6 on the Beaufort scale, a "strong breeze" and three kites were tried, not one

FIG. 1—*continued.*

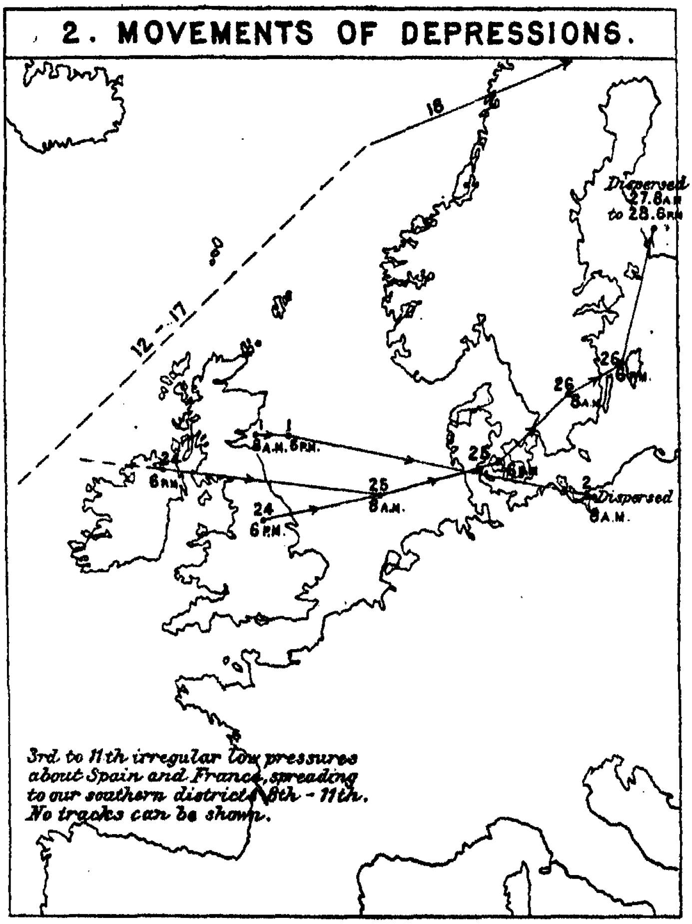


A closed curve thus () denotes a temperature inversion.

could be got to rise above 2000 feet (670 metres). Fig. 2 shows how few depressions passed near Crinan during the six weeks of observation, and the absence of west and north-west winds to some extent defeated the object of the investigation, namely, a determination of the oceanic temperature gradient, but on the other hand the unusual prevalence of land winds have afforded the means of comparison with the preceding years when different conditions prevailed.

The table of gradients and the diagram indicate a departure from the preceding results in the steeper gradient for the first 500 metres, and in the comparatively numerous temperature inversions between 1000 and 2000 metres. A steep gradient in the daytime in summer is a characteristic of continental observations,* and it is not surprising that the prevalence

FIG. 2.



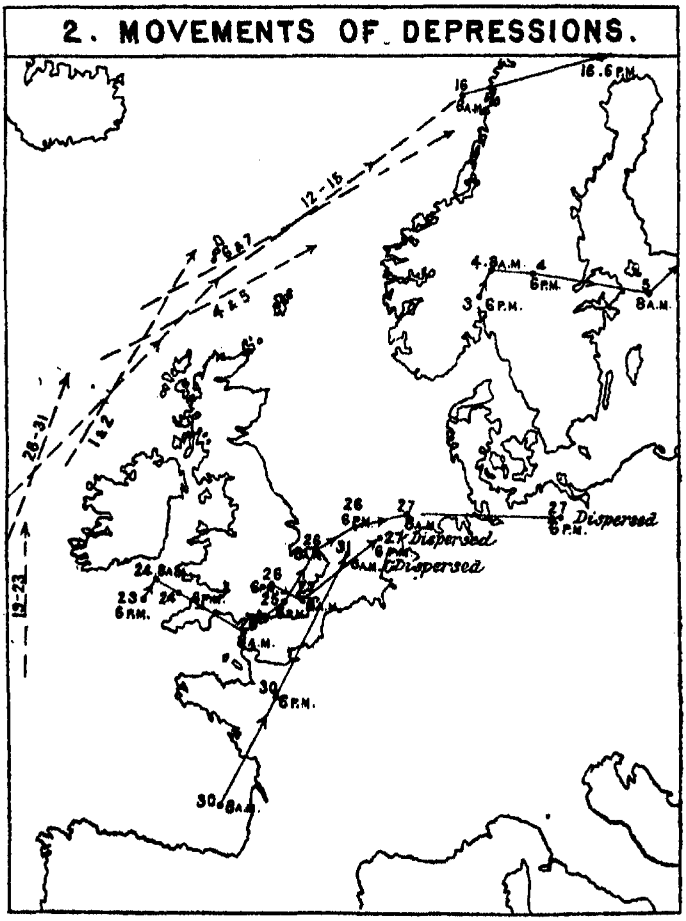
JUNE, 1904.

of land winds should produce a steeper gradient. Such marked inversions of temperature have not been observed at Crinan in the preceding years of observation, and the inference certainly is that temperature inversions between 500 and 2500 metres are unusual off the Scotch coast with westerly

* The gradient 1·10 for the first 500 metres of the United States Weather Bureau ascents (see table) is beyond the adiabatic rate for dry air. These were chiefly, if not entirely, day-time ascents in the summer.

winds. It is noteworthy that if we take the average gradient up to 1500 metres (5000 feet) it has been practically the same in each of the three summers; although the observations at Crinan, or rather over the sea in the neighbourhood of Crinan, in 1902 and 1903 were, on account of the prevalence of westerly winds, equivalent to observations over the North Atlantic.

Fig. 2—*continued.*



July, 1904.

Comparison with Ben Nevis.

Table B shows the relation between the temperature on the summit of Ben Nevis and the temperature of the free air about 60 miles to the south-west at the same time. The temperature given by the kite is the mean of those obtained on the ascent and descent, and the Ben Nevis temperature

is that of the exact hour that stands nearest to the middle of the kite ascent. The figures are in striking contrast with those relating to the two preceding years. For the first time the temperature at the kite was occasionally found to be below that on the summit of the mountain. Owing to the use of larger kites and a lighter meteorograph, it was possible to get observations on days on which in the summer of 1902 it would have been impossible, on account of the lightness of the wind, to reach the level of Ben Nevis, but it is obvious that this is not a sufficient explanation, since the days on which Ben Nevis was the warmer were not days of exceptional calmness.

Table B.—Difference of Temperature between Ben Nevis and a Kite at the same level about 60 miles to the south-west.

Date.	Kite.	Ben Nevis.		Difference, B.N. − kite.	Weather under kite.
		Dry.	Wet.		
June 21, 12	2·4 C.	1·2	1·2	−1·2	W., 4, *b*
22, 12	3·3	2·5	2·5	−0·8	W., 4, *c*
24, 12	5·5	3·2	3·2	−2·3	S.E., 2, *c*
28, 12	6·0	8·2	5·8	2·2	E.S.E., 1, *c*
29, 12	7·5	10·5	7·6	3·0	S.E., 3, *b*
30, 2 P.M.	8·0	11·1	6·6	3·1	S.E., 4, *c*
July 1, 3 "	3·6	2·8	2·8	−0·8	S., 6, *r*
2, 12	*I. 2·4	2·4	2·4	0·0	S.W., 3, *r*
4, 11 A.M.	2·4	1·7	1·7	−0·7	S.S.W., 3, *r*
5, 12	I. 5·0	2·3	2·3	−2·7	W., 2, *c*
6, 11 A.M.	8·8	7·8	7·8	−1·0	S.S.W., 7, *r*
7, 12	1·4	0·6	0·6	−0·8	S.W., 6, *c*
8, 11 A.M.	4·3	3·9	3·9	−0·4	S.S.W., 7, *r*
13, 2 P.M.	4·9	7·1	7·1	2·2	S.S.W., 4, *b*
15, 11 A.M.	6·7	6·6	6·6	−0·1	S.S.W., 5, *c*
16, 3 P.M.	I. 5·0	3·2	3·2	−1·8	W.S.W., 3, *b*
19, 12	8·8	12·3	8·7	3·5	S.S.E., 2, *b*
21, 12	7·5	7·5	7·5	0·0	S.S.E., 2, *b*
22, 11 A.M.	9·0	7·5	7·5	−1·5	S.E., 1, *c*
25, 12	10·1	8·5	7·5	−1·6	E., 1, *b*
26, 12	I. 5·0	7·4	6·7	2·4	E. by S., 3, *c*
28, 12	I. 11·5	11·4	8·6	−0·1	E., 1, *c*

* Denotes a temperature inversion at about the height of Ben Nevis.

Explanation of letters:—*b*. Blue sky. *c*. Detached clouds. *o*. Overcast. *r*. Rain.

With the exception of July 13, on each day on which the mountain was the warmer the air on its summit was dry, and mostly very dry; with the same exception also the winds were south-east. It is noticeable too that the differences were small on the five rainy days given in the table. In the preceding paper it was suggested that the lower temperature of the mountain was due to the adiabatic cooling of the air as it was forced up the mountain slope by the prevailing westerly winds, the temperature gradient so produced being greater than the ordinary gradient in the

free air ; and the results obtained in 1904 seem to me to support this conclusion.

On rainy days the gradient in the free air, at least throughout the vertical region in which rain is forming, must be the adiabatic one, for the dynamic cooling of an ascending current is the only admissible cause of any but the lightest rain, and on such days the adiabatic cooling should produce much the same temperature on the mountain and in the free air. With a fairly clear sky and a south-east wind the air would be warmed, by contact with the ground before reaching Ben Nevis, and it is probable that any excess of moisture it may have had originally would be deposited on the slopes of the mountains lying to the east and south. A westerly wind, on the other hand, could not be dry, coming from the sea, and, as is usually the case, would produce clouds and a saturated condition on Ben Nevis.

Different instruments were used in each year and the question arises whether these differences are due to instrumental errors. An error may arise from an incorrect base line or from an incorrect scale. The former error is excluded by the fact that in each year the instruments were compared almost daily with verified thermometers, and an error exceeding half a degree must have been detected. Some error from the second cause is certainly possible, but it could not have been large. The usual range of temperature at Crinan during the summer is small, the temperature during the daytime seldom exceeding 20° C. or falling below 12° C., so that an error in the scale amounting to 1 in 16 may, perhaps, have escaped detection. No means existed of obtaining low temperatures artificially, and the meteorograph used in 1902 was never compared with a thermometer at temperatures outside the ordinary range of the district. That used in 1903 was obtained from M. Teisserenc de Bort, he sent me particulars as to the scales, and I do not doubt that they were correct.

I tested the meteorograph used in 1904 over the whole range of likely temperatures before going to Crinan, and although the zero of these instruments is liable to alteration, the scale is not. Hence, on the whole, I consider that while an instrumental error of something under 1° C. is possible, it is hardly likely that the differences are due to this cause. The fact that temperature inversions were far more prevalent in 1904 than in the previous years shows that the average weather conditions were different.

Temperature Gradients and Weather Classification.

The temperature gradients over the sea on the west coast of Scotland have now been ascertained on 68 days, and it seems desirable to separate and arrange them according to the weather in which they were observed.

The classification of weather is that suggested by Dr. Shaw in a paper read before the Scottish Meteorological Society in December, 1904.*

South-easterly Type.

A pressure distribution favourable for winds between east and south for the 24 hours to which the observations refer.

South-westerly Type.

Favourable for winds between south and west.

North-westerly Type.

Favourable for winds between north and west.

North-easterly Type.

Favourable for winds between north and east.

Variable Anticyclonic.

Variable winds during the prevalence of an anticyclone.

Variable Cyclonic.

With sequence of winds incidental to the passage of a cyclone.

A cyclonic region is taken as one in which the isobars are concave to the low-pressure region and probably form closed curves round the low-pressure centre. An anticyclone is one in which they are concave to the high-pressure region.

In cases where the isobars are approximately straight the region is classed as intermediate, and it has been necessary to add the term "transitional" for cases where for the interval in question the type is changing.

The results are shown in Table C, next page.

It must be borne in mind that these gradients refer to the daytime and to the summer. Inasmuch as the daily temperature variation on the sea is very trifling, or perhaps even non-existent away from the ship which carries the thermometers, we may take the conditions as being those prevailing, without reference to the hour, during the summer months. There is no reason to suppose they would be different in the winter, but in the absence of direct information no definite assertion can be made.

The observations from strata over 2000 metres have been omitted, as they are not sufficiently numerous to lead to any conclusion, and, indeed, the figures given in the table are such, that when considered in connection

* 'Journal of the Scottish Meteorological Society,' Third Series, Nos. 20 and 21.

with the number of observations, it would not be unreasonable to say that the differences are of such an order that they may be purely chance ones.

Table C.—Gradients, in metres, at Crinan for various Types of Weather.

No. of observation.	0 to 500.	500 to 1000.	1000 to 1500.	1500 to 2000.	Mean.
		Cyclonic.			
28	0·48	0·60	0·46	0·54	0·52
		Intermediate.			
31	0·76	0·54	0·42	0·52	0·56
		Anticyclonic.			
4	0·66		Observations wanting.		
		Transition.			
5	0·54	0·56	0·50	0·40	0·50
		North-westerly type.			
24	0·66	0·62	0·44	0·32	0·51
		South-westerly.			
21	0·72	0·60	0·40	0·58	0·58
		South-easterly.			
11	0·58	0·48	0·26	0·58*	0·44
		North-easterly.			
		Not sufficient observations.			
		Variable.			
7	0·46	0·60	Observations wanting.		0·53

* Only two observations and therefore not included in the mean.

The noticeable points are the small gradient from 0 to 500 metres in cyclonic weather compared with the high value for the same strata for the intermediate type, and the low value from 1000 to 1500 for the south-easterly type. This latter result is due to the temperature inversions occurring in 1904, but it is not possible to say definitely whether a small gradient is the usual accompaniment of the south-easterly type. It will be necessary to obtain more observational information before forming a definite opinion on the whole question.

The absence of observations for the higher strata with the anticyclonic and variable types shows plainly that these types are accompanied by very light winds at a small height, since the want of observations can only be due to the want of sufficient air motion to raise the kites. The one safe conclusion to be drawn seems to be that the different types of weather are not characterised by any peculiarity of gradient, neither is there any reason

for expecting that they should be, except in special cases. Anticyclonic conditions, it is well known, produce during a winter's night on land a sharp inversion in the lower strata, and the same conditions during a summer's day produce the steepest gradient that the conditions of equilibrium will admit of, but the two results tend to cancel each other and give on the whole an average gradient. It is almost certain, too, that neither result occurs over the open sea.

The gradient in the precise region in which rain is forming, or, more strictly, in which vapour is being condensed, must in general be the adiabatic gradient for saturated air, but, compared with the volume of the whole mass of the atmosphere, such regions are very small indeed. Let us call such a region of ascending air A. Whether this region be the rainfall area of a large cyclonic disturbance, or merely that of a small local shower from an isolated cloud, there must be associated with it a compensating region, B, in which the air is descending. Ferrel has pointed out that owing to the latent heat of condensation the air in and over the region A must be relatively warm, and that this warmth will suffice to produce the circulation.

But there is a point which may be easily overlooked. The adiabatic rate for dry air, such as will be found in B, is far greater than for the saturated air which is found in A, and if the passage from A to B be a purely adiabatic one, the temperature in B will be far higher than in A, a condition which would immediately reverse the direction of the circulation. Hence the circulation cannot occur, unless it be a forced one, and not a convectional effect. Where A is a mountain slope up which a wind is blowing, we have a forced circulation, with a foehn wind in the region B, but in the free atmosphere it is very difficult to see what source of power there can be to produce a forced circulation. It seems more likely that the change from A to B is not adiabatic, and that the region B is very extensive when compared with A and perhaps some considerable distance from it, also that the air which is descending in B, to replace in the lower strata that which is rising in A, is air that ascended some time since and has had time to become cool by admixture with other air and perhaps also to some extent by radiation. If this is the case there will be no special gradient produced by the condensation of vapour anywhere but in the small and limited regions where the condensation is occuring. In support of the above suggestions I may quote the results* obtained by unmanned balloons, which show that over cyclonic regions at

* "Über die Temperaturabnahme mit der Höhe bis zu 10 km., nach den Ergebnissen der internationalen Ballonaufsteige;" 'Sitzungsberichte der Kaiserl. Akademie der Wissenschaften in Wien, Mathem.-naturw. Klasse,' vol. 113, Abt. IIa, May, 1904.

great heights the air is relatively warm, and over anticyclonic regions relatively cold.

The temperature inversions are easily explained on the same hypothesis. The almost invariable accompaniment of an inversion of temperature has been, both at Crinan and Oxshott, a large decrease in the relative humidity, and this leads to the conclusion that the warmth is due to the compression of descending air rather than to the presence of a horizontal current of air from a warmer quarter. Inasmuch as descending air is warmed 1° C. for every 100 metres of its descent, it must soon become specifically lighter than the air around it and will tend to spread out as a sheet of warm air; just as wine or spirits may be made by careful pouring to form a separate layer on the surface of water. Hence each ascending current that is accompanied by much condensation of vapour should tend to form an inversion elsewhere.

No doubt temperature inversions may be formed by a warm current of air coming from a warm quarter and overlying a cold stratum, but from my own experience I do not think this method to be of frequent occurrence in England.

It is probably a mistake to suppose that our present instruments and methods can detect changes of temperature which may well suffice to set in motion large masses of the atmosphere. The vertical component of the velocity in the ordinary cyclone is extremely small—a few hundred feet per hour perhaps. This may be inferred from the rate of rainfall, and also from the velocity and incurvature of the winds. Now on a non-rotating earth such an updraft would certainly require only a very small excess of temperature to produce it. In actual practice the temperature depends chiefly upon the direction of the wind, the northerly winds of the cyclone being cold and the southerly warm, and the changes so produced may well mask all other effects.

Gradients at Oxshott.

The temperature gradients at Oxshott are given in Table D. It has been previously stated that the majority of these ascents were not made with the definite idea of obtaining information, but rather with that of testing the apparatus. In consequence the kites have generally been allowed to rise as fast as they would, and have also been drawn in rapidly. Under these circumstances while the average gradient is perfectly clear, the gradient for short steps is not so easy to determine. For this reason it is given in the table in steps of 1000 instead of in steps of 500 metres. But while this applies to the individual ascents, it is not likely that there is much error in the average for each step of 500 metres, and these values have therefore been inserted in Table A for the sake of comparison.

Table D.

Date.	Gradient.		Wind.			Type of weather.	
			Below.		Above.		
	0—1000.	1000—2000.	Direction.	Force.	Direction.		
1904.							
Feb. 19...	0·40	—	S.W.	6	W.	T.	
22...	0·90	—	N.N.W.	3	—	N.W.,	I
Mar. 10...	0·60	—	N. by E.	5	—	N.E.,	A
18...	0·68	—	S.W.	3	W.	S.W.,	A
22...	1·04	—	S.W.	4	W. by S.	N.W.,	I
23...	0·95	—	N. by E.	6	—	N.W. to N.E.,	I
30...	1·08	—	S.W.	3	—	N.W.,	I
Apr. 4...	1·10	—	W.S.W.	7	—	N.W.,	I
6...	0·85	—	W.S.W.	3	—	N.W.,	I
7...	0·75	—	W.N.W.	6	—*	N.W.,	I
8...	1·10	—	W.S.W.	6	N.N.W.	N.W.,	I
13...	0·43	—	S.	7	S. by W.	S.W.,	I
28...	0·84	—	W.S.W.	3	—	S.W.,	A
29...	0·33	—	S.S.W.	4	W.S.W.	S.W.,	I
May 13...	1·03	—	W.S.W.	6	S.W.	S.W.,	I
Sept. 30...	0·64	0·00	S.S.W.	4	S.W. by W.†	S.W.,	I
Oct. 3...	0·73	—	N.E.	3	—	N.E.,	A
5...	0·32	—	W.S.W.	4	W. by N. at 700‡	S.W.,	C
6...	0·73	—	W.S.W.	6	W. by N.	N.W.,	I
7...	0·60	0·48	N.W.	5	—	N.W.,	I
8...	0·70	—	N.W.	6	—	N.W.,	I
Nov. 3...	0·28	—	W.	3	N.W.	T.	
8...	0·74	—	W.S.W.	7	W.	N.W.,	I
30...	0·48	—	W.S.W.	4	N.W.	N.W.,	I
Dec. 1...	0·40	—	W.S.W.	3	N.W.§	S.W.,	I
5...	0·84	—	S.W.	6	—	S.W.,	I
13...	0·53	—	N.W.	5	N. by W.	N.W.,	C
16...	0·58	—	S.W.	6	W.S.W.	S.W.,	I
17...	0·72	—	S.W.	7	W.S.W.	S.W.,	I
29...	0·42	—	W.	5	—	N.W.,	A
1905.							
Jan. 4...	0·63	0·46	W.S.W.	6	W.N.W.	N.W.,	I
5...	0·66	0·46	W.	5	N.W.‖	N.W.,	C
28...	0·44	—	W.	2	N.W. by W.¶	**N.W.,	A
30...	0·29	—	W.S.W.	4	—	N.W.,	I
Feb. 3...	0·61	—	W.S.W.	7	N.W.	N.W.,	I
4...	0·56	—	S.W. by W.	5	W.N.W.††	N.W.,	I
10...	0·62	0·22	W.S.W.	3	W.N.W.‡‡	N.W.,	I
17...	0·70	—	W.	5	W.N.W.	N.W.,	I

* Slight inversion at 900 m.
† Very dry at 1500 m., 30 per cent.
‡ W.S.W. at 1000. Inversion 4° C. at 1200 m., 20 per cent. humidity.
§ Inversion 2° C. at 900 m.
‖ Wind very strong at 800 m., light at 1700 m.
¶ Inversion 7° C. at 900 m.; very dry.
** Unusually high barometer, 30·90 and over.
†† Wind very strong at 600 m.
‡‡ Inversion at 1800 m., 40 per cent. humidity.

Table D—*continued.*

Date.	Gradient.		Wind.			Type of weather.
			Below.		Above.	
	0—1000.	1000—2000.	Direction.	Force.	Direction.	
1905.						
Feb. 18...	0·46	−0·08	S.W.	6	W.N.W.*	S.W., C
Mar. 2...	0·72	0·00	N.N.E.	6	N.E.	N.E., I
3...	0·90	—	N.	3	N. by E.	N.E., A
6...	0·69	0·00	W.S.W.	3	N.W.	N.E., C
8...	0·58	—	S.S.W.	5	S.W.	S.W., C
17...	0·67	—	S.S.W.	6	S.W. by W.	S.W., I
25...	0·64	—	S.W.	3	W.S.W.	S.W., C
28...	0·63	—	S.W.	5	W.S.W.	S.W., I
29...	0·73	0·32	S.W.	5	W.S.W.	S.W., C
31...	0·67	0·67	W. by S.	5	W.N.W.	N.W., A
Apr. 1...	0·90	0·23	S.S.W.	3	W.S.W.	T. A. to C. S.W.
4...	0·74	0·47	W.	4	N.W.	S.W., C
7...	1·06	0·65	N.W.	4	N.W. by W.	V. C
13...	0·77	0·72	S.S.E.	5	—	S.E., C
17...	0·28	—	N.E.	7	E.N.E.	N.E., C
20...	0·64	—	N.N.E.	5	—	N.E., I
24...	0·48	—	W.N.W.	3	—	N.E., C
25...	0·80	0·30	S.W.	4	W. by S.	S.W., I
27...	0·73	0·00	S.W.	5	W.†	S.W., I
28...	0·63	—	S.S.W.	7	—	S.W., C
May 5...	0·58	0·32	N.N.E.	4	E.N.E.	N.E., I
8...	1·00	—	N.N.W.	3	—	N.W., I
June 1...	0·85	0·70	S.W. by W.	5	W. by N.	S.W., A
14...	0·69	—	N.E.	4	—	S.E., I
19...	0·79	0·62	S.W.	3	W.S.W.	S.W., I
Aug. 10...	0·93	—	S.W.	4	—	S.W., C
11...	0·78	—	W.	3	—	N.W., C
16...	0·70	—	E.N.E.	5	—	N.E., I
18...	1·00	—	S.	6	—	S.W., C
21...	1·05	—	S.S.W.	3	S.W.‡	S.W., I
26...	1·10	0·07	S.S.W.	3	S.S.W.§	S.W., C
30...	0·53	0·50	N.W.	3	N.N.W.	N.W., C
Sept. 2...	0·65	—	W.	4	—	N.W., I
18...	1·00	0·00	E.N.E.	6	E. by N.‖	N.E., C
23...	0·62	0·73	E.N.E.	4	—	N.E., C
28...	0·95	0·54	E.N.E.	5	—	N.E., C
30...	0·90	0·08	N.	4	N. by W.¶	N.W., I

The wind force is estimated from the pull and behaviour of the kites, and from the daily weather chart. The anemometer is too sheltered to be reliable.

* Upper wind weak.
† Inversion at 1100 m.
‡ Gradient 10°·5 m. to 900 m., then inversion 4°·5 C. Humidity 40 per cent. at 1200 m.
§ Inversion with dryness 40 per cent. at 1800 m.
‖ Inversion at 1100 m. Humidity 30 per cent.
¶ Inversion 4°·5 C. at 1700 m.

No ascent of a less height than 666 metres is included in Table D. Where one gradient only is tabulated this is the average from the surface to the

highest point reached, which may have been anywhere between 666 and 1500 metres.

It will be noticed that nearly all the ascents have been made with winds from some westerly point; this is because it is difficult to start a kite with an east wind owing to a belt of trees and houses which lie to the west. But few ascents are tabulated for the summer months, owing partly to my absence in 1904, and partly to want of sufficient wind in 1905. In general there is not sufficient wind for kite flying in the south-east of England during the summer.

There are in the table a few cases in which the adiabatic rate for dry air has been exceeded. This appears to me to be an impossibility, but it is unnecessary to discuss the question, since it has been fully gone into by Mr. H. H. Clayton,* who gives what seems to be a satisfactory explanation.

The gradients at Oxshott are steeper than those at Crinan for the first thousand metres, but less steep for the second thousand. The first fact is easily explained, for the daily range of temperature is confined to the lower layers of air, it is larger inland and the Oxshott ascents have all been made in the daytime, when the surface temperature is near its maximum value. It is somewhat remarkable that the average gradient taken up to 2000 metres is almost identical in each case, although the values for the separate steps differ considerably. Can it be that there is some form of compensation, so that if in one step of 500 metres the gradient is increased, the same cause or set of causes tend to lower it in another step?

We have the following figures:—

Average to 2000 metres at Crinan in 1902..............	0·54
" " " 1904..............	0·53
" " Oxshott, 1904 and 1905 ...	0·50

The last step is missing for Crinan on August, 1903, but up to 1500 m. it was 0·61. Perhaps this increased value is associated with the bad weather of that time.

Convection Currents.

It very soon becomes obvious to anyone engaged in flying kites that there are great irregularities in the direction of motion of the air, both vertically and horizontally. The horizontal variation is shown beyond dispute by the changing azimuth of the kite, and the vertical variation may be inferred but cannot be predicated with absolute certainty by the changing altitude. Thus, if a kite is flying with 5000 feet of line the altitude may vary from 20° to

* 'Annals of the Astronomical Observatory of Harvard College,' vol. 58, part 1, pp. 14, 15.

50° or even 60°, but these angles, at least up to 55°, may be dependent on the velocity only and not on any vertical component. It has happened more than once that the angle of a kite with 3000 feet of wire has exceeded 70°, and these instances, which were all associated with the presence of a large massive cumulus cloud, show undoubtedly the existence of a strong vertical component. It is seldom, however, that the angle exceeds one which might be produced naturally by a strong horizontal current at the level of the kite. Hence on a day when the altitude is constantly changing through a large angle, although one feels certain that convection currents produce a large part of the change, it is not possible to say how large a part. These currents are most likely produced by changes of temperature, and it is of interest to ascertain the precise difference of temperature between the falling and rising currents. With this object in view I have been carefully over the records obtained on such days, but with one exception have failed to find any important difference in the temperature at a definite level when the kite was rising, and when it was falling.

This exception occurred on June 19, 1905, at 4.20 P.M. A kite was flying at the end of 8000 feet of wire, at a height of about 4500 feet, hidden behind a large cumulus cloud, the strain upon the wire being steadily maintained at 40 to 50 lbs. The tension in the wire rose rapidly but without jerks to 200 lbs., and remained at this value for about 2½ minutes; it fell back again in the same manner to 50 lbs., at which value it remained steady. On winding in the kite the temperature trace was found to be somewhat blotted at the critical point, as, unfortunately, too much ink had been put on the pen, but it was sufficiently distinct to give the following data. In the course of a few minutes the kite had risen and fallen again through a vertical height of 1300 feet. This change of height was associated with a rise of temperature of 4° C., but the temperature was not symmetrical with regard to the height. About three minutes before the updraft occurred the temperature was 5° C. at a height of 4500 feet, it then rose steadily to 9° C. at 5800 feet, the highest point reached, and declined to 7° C. as the kite fell back to its original level. During the same period the relative humidity rose from 90 to 100 per cent., and fell back to 95 per cent.

In this particular instance there is little doubt that the kite was caught in a powerful ascending current. The rise of temperature with elevation could hardly be due to the ordinary inversion, because it was associated with an increase of the humidity, whereas a temperature inversion is generally accompanied by great dryness. It is probable that these currents are sometimes responsible for the breaking away of a kite, for this is not by any means the first time a similar phenomenon has been noted, but it is the

first time that the kite has escaped undamaged and the trace been decipherable. In general a steady and uniform wind is associated with steady temperature conditions, but when the temperature at a given height is subject to much fluctuation, so that the meteorograph registers different temperatures each time it passes through that height, the wind also is usually variable in direction and velocity, but these latter conditions are not necessarily accompanied by a steep temperature gradient.

The Calculation of Ellipsoidal Harmonics.

By Sir W. D. Niven, K.C.B., V.-P.R.S.

(Received March 22,—Read March 29, 1906.)

1. The object of this note is to show how ellipsoidal harmonics of the fourth, fifth, sixth, and seventh degrees may be calculated. Some of the fourth and fifth degrees are easily found, depending as they do upon the solution of a quadratic equation. When, however, the type of the harmonic of the fourth degree is $\Theta_1 \Theta_2$ where, if we employ Greek letters for current co-ordinates,

$$\Theta_1 = \frac{\xi^2}{a^2+\theta_1}+\frac{\eta^2}{b^2+\theta_1}+\frac{\zeta^2}{c^2+\theta_1}-1,$$

$$\Theta_2 = \frac{\xi^2}{a^2+\theta_2}+\frac{\eta^2}{b^2+\theta_2}+\frac{\zeta^2}{c^2+\theta_2}-1,$$

and, in order that $\Theta_1 \Theta_2$ may satisfy Laplace's equation, the quantities θ_1, θ_2 are to be found from

$$\frac{1}{a^2+\theta_1}+\frac{1}{b^2+\theta_1}+\frac{1}{c^2+\theta_1}+\frac{4}{\theta_1-\theta_2} = 0, \tag{1}$$

$$\frac{1}{a^2+\theta_2}+\frac{1}{b^2+\theta_2}+\frac{1}{c^2+\theta_2}+\frac{4}{\theta_2-\theta_1} = 0, \tag{2}$$

the elimination of θ_2 leads to a sextic equation in θ_1. Since, however, the roots occur in pairs as in (1) and (2), if we put

$$\theta_1+\theta_2 = 2u, \qquad \theta_1-\theta_2 = 2v, \tag{3}$$

the equations for u and v derived from (1) and (2) will be of lower degrees than the sixth.

When the substitutions (3) are made in (1) the latter becomes

$$\frac{1}{a^2+u+v}+\frac{1}{b^2+u+v}+\frac{1}{c^2+u+v}+\frac{2}{v} = 0, \tag{4}$$

and equation (2) will be the same with the sign of v changed.

Multiplying up in (4), arranging in powers of v and putting P_1 for the sum of the quantities a^2+u, b^2+u, c^2+u, P_2 for their sum taken two and two and P_3 for their product, we find

$$5v^3+4P_1v^2+3P_2v+2P_3 = 0. \tag{5}$$

As this equation is true when $-v$ is entered for v, it follows that

$$v^2 = -\tfrac{3}{5}P_2 = -\tfrac{1}{2}\frac{P_3}{P_1}. \tag{6}$$

Hence
$$5P_3 = 6P_1P_2. \tag{7}$$

This is a cubic in u which when solved leads to the values of v corresponding to those of u.

2. To express the cubic in a convenient form let a, b, c be in ascending order of magnitude and write

$$b^2-a^2,\ a^2+u,\ v = (p,\ x,\ y)\,(c^2-a^2). \tag{8}$$

Then will

$$b^2+u,\ c^2+u = (x+p,\ x+1)\,(c^2-a^2), \tag{9}$$

and

$$\left.\begin{aligned} P_1 &= (3x+1+p)\,(c^2-a^2),\\ P_2 &= [3x^2+2\,(1+p)\,x+p]\,(c^2-a^2)^2,\\ P_3 &= [x^3+(1+p)\,x^2+px]\,(c^2-a^2)^3. \end{aligned}\right\} \tag{10}$$

Entering these values in (7), we obtain

$$49x^3+49\,(1+p)\,x^2+(12+37p+12p^2)\,x+6p\,(1+p) = 0. \tag{11}$$

As the roots of this equation are all real the equation may be solved by the method given in treatises on trigonometry.

By putting

$$x = -\tfrac{1}{3}(1+p)+X, \tag{12}$$

we obtain
$$X^3-qX-r = 0, \tag{13}$$

where $\quad q = \tfrac{13}{147}(1-p+p^2), \qquad r = \tfrac{5}{1323}(1+p)(2-5p+2p^2).$

The solution is then

$$X = [\cos\alpha,\ \cos(\tfrac{2}{3}\pi+\alpha),\ \cos(\tfrac{2}{3}\pi-\alpha)]\sqrt{\frac{4q}{3}},$$

where
$$\cos 3\alpha = 4r\sqrt{\frac{27}{64q^3}}.$$

The corresponding expression for the difference of the roots is given by

$$y^2 = -\tfrac{3}{5}[3x^2+2\,(1+p)\,x+p] = \tfrac{1}{5}(1-p+p^2-9X^2). \tag{14}$$

The equations (12), (13), (14) completely determine the values of θ and they show that there are three harmonics of the type considered.

3. Harmonics of the types $(\xi,\ \eta,\ \zeta,\ \eta\zeta,\ \zeta\xi,\ \xi\eta,\ \xi\eta\zeta)\ \Theta_1\ \Theta_2$ may be calculated

in similar fashion, but the working being in all respects like that in §§ 1, 2 need not be repeated. Equations (1) and (2) will, of course, be different. For instance, if we are considering $x\Theta_1\Theta_2$ the first term in equations (1) and (2) must be multiplied by 3, and if we are considering $yz\,\Theta_1\Theta_2$ the second and third terms must be multiplied by 3.

The results for the seven harmonics described above will now be stated in the final forms suitable to the trigonometrical solution of §2, *i.e.*, in the forms similar to those expressed by the three equations (12), (13), (14).

$\xi\Theta_1\Theta_2$.—

$$27x = -10(1+p)+27\mathrm{X};$$
$$(27\mathrm{X})^3-3(28-43p+28p^2)(27\mathrm{X})-(1+p)(160-355p+160p^2) = 0;$$
$$77(27y)^2 = 27^2(16-23p+16p^2)-[11(27\mathrm{X})-2(1+p)]^2.$$

$\eta\Theta_1\Theta_2$.—

$$27x = -(10+7p)+27\mathrm{X};$$
$$(27\mathrm{X})^3-3(28-13p+13p^2)(27\mathrm{X})+(2p-1)(160+35p-35p^2);$$
$$77(27y)^2 = 27^2(16-9p+9p^2)-[11(27\mathrm{X})+4p-2]^2.$$

$\zeta\Theta_1\Theta_2$.—

$$27x = -(7+10p)+27\mathrm{X};$$
$$(27\mathrm{X})^3-3(13-13p+28p^2)(27\mathrm{X})-(2-p)(35-35p-160p^2);$$
$$77(27y)^2 = 27^2(9-9p+16p^2)-[11(27\mathrm{X})+4-2p]^2.$$

$\eta\zeta\Theta_1\Theta_2$.—

$$33x = -10(1+p)+33\mathrm{X};$$
$$(33\mathrm{X})^3-21(4-p+4p^2)(33\mathrm{X})-(1+p)(160-463p+160p^2)= 0;$$
$$117(33y)^2 = 33^2(16-7p+16p^2)-[13(33\mathrm{X})+2(1+p)]^2.$$

$\zeta\xi\Theta_1\Theta_2$.—

$$33x = -(10+13p)+33\mathrm{X};$$
$$(33\mathrm{X})^3-21(4+7p-7p^2)(33\mathrm{X})-(1-2p)(160+143p-143p^2) = 0;$$
$$117(33y)^2 = 33^2(16-25p+25p^2)-[13(33\mathrm{X})+2-4p]^2.$$

$\xi\eta\Theta_1\Theta_2$.—

$$33x = -(13+10p)+33\mathrm{X};$$
$$(33\mathrm{X})^3-21(7-7p+4p^2)(33\mathrm{X})-(2-p)(143-143p-160p^2) = 0;$$
$$117(33y)^2 = 33^2(25-25p+16p^2)-[13(33\mathrm{X})-4+2p]^2.$$

$\xi\eta\zeta\Theta_1\Theta_2$.—

$$39x = -13(1+p)+39\mathrm{X};$$
$$(39\mathrm{X})^3-147(1-p+p^2)(39\mathrm{X})-143(1+p)(2-5p+2p^2) = 0;$$
$$33y^2 = 5(1-p+p^2-9\mathrm{X}^2).$$

4. The results given in the preceding section exhaust all the cases in which the harmonic has two Θ factors. We pass on to the harmonic of sixth degree with three such factors

$$\Theta_1\Theta_2\Theta_3.$$

We have to solve the following set of equations:—

$$\frac{1}{a^2+\theta_1}+\frac{1}{b^2+\theta_1}+\frac{1}{c^2+\theta_1}+\frac{4}{\theta_1-\theta_2}+\frac{4}{\theta_1-\theta_3}=0. \tag{15}$$

$$\frac{1}{a^2+\theta_2}+\frac{1}{b^2+\theta_2}+\frac{1}{c^2+\theta_2}+\frac{4}{\theta_2-\theta_1}+\frac{4}{\theta_2-\theta_3}=0. \tag{16}$$

$$\frac{1}{a^2+\theta_3}+\frac{1}{b^2+\theta_3}+\frac{1}{c^2+\theta_3}+\frac{4}{\theta_3-\theta_1}+\frac{4}{\theta_3-\theta_2}=0. \tag{17}$$

Following the method of § 1, we now put

$$\left.\begin{aligned}\theta_1+\theta_2+\theta_3 &= 3u,\\ \theta_2-\theta_3 &= 3r,\\ \theta_3-\theta_1 &= 3s,\\ \theta_1-\theta_2 &= 3t.\end{aligned}\right\} \tag{18}$$

And, with these substitutions, equation (15) becomes

$$\frac{1}{a^2+u+t-s}+\frac{1}{b^2+u+t-s}+\frac{1}{c^2+u+t-s}+\frac{4}{3}\left(\frac{1}{t}-\frac{1}{s}\right)=0. \tag{19}$$

Or, on multiplying up,

$$4(t-s)P_3+[4(t-s)^2-3ts]P_2+2(t-s)[2(t-s)^2-3ts]P_1 +(t-s)^2[4(t-s)^2-9ts]=0. \tag{20}$$

Similarly,

$$4(r-t)P_3+[4(r-t)^2-3rt]P_2+2(r-t)[2(r-t)^2-3rt]P_1 +(r-t)^2[4(r-t)^2-9rt]=0. \tag{21}$$

The third equation need not be written.

In these equations P_1, P_2, P_3 have the same meanings as in § 1, except that u is now the mean of three θ's instead of two. As the determination of r, s, t from the equations appears not to be practicable, P_1, P_2, P_3 will be found in terms of r, s, t.

First eliminate P_3 from (20) and (21) observing that $r+s+t=0$. There results

$$(3t^2+2rs)P_2+2(s-r)(r-t)(t-s)P_1=6(r-t)(t-s)(t^2-rs). \tag{22}$$

It will be noticed that t^2-rs is a symmetrical function of r, s, t, for since $r+s+t=0$ it is $\frac{1}{2}(r^2+s^2+t^2)$. This quantity, as it appears frequently in the work, will be denoted by V.

Combining (22) with another equation of the same form, with the letters interchanged, we find

$$P_2 = -\tfrac{18}{5}V, \qquad (23)$$

$$(s-r)(r-t)(t-s)P_1 = -\tfrac{3}{5}V^2. \qquad (24)$$

To obtain P_3 subtract (21) from (20), still making use of $r+s+t=0$. There results

$$4P_3 - 5(s-r)P_2 + 16VP_1 - (s-r)(21t^2+11V) = 0. \qquad (25)$$

Writing two other similar equations and adding the three we find, on removing the factor 3,

$$4P_3 + 16VP_1 + 7(s-r)(r-t)(t-s) = 0. \qquad (26)$$

The elimination from (23), (24), (26) of V and the product of the differences of r, s, t may now be made and we obtain

$$48P_1(9P_3 - 10P_1P_2) - 35P_2^2 = 0. \qquad (27)$$

This is a biquadratic in u, showing that there are four harmonics of the type under discussion.

The equation giving r, s, t must now be found. It must clearly be of the form

$$X^3 + \tfrac{5}{18}P_2X + R = 0, \qquad (28)$$

where

$$r+s+t = 0,$$

$$st+tr+rs = -\tfrac{1}{2}(r^2+s^2+t^2),$$

$$= \tfrac{5}{18}P_2,$$

and R is to be determined from the condition

$$(s-r)(r-t)(t-s) = -\tfrac{5}{108}\frac{P_2^2}{P_1}.$$

Now the square of the product on the left is, by a known theorem, equal to

$$-27(\alpha^3 + \tfrac{5}{18}P_2\alpha + R)(\beta^3 + \tfrac{5}{18}P_2\beta + R),$$

where α, β are the roots of

$$\frac{d}{dX}(X^3 + \tfrac{5}{18}P_2X + R) = 0,$$

or,

$$3X^2 + \tfrac{5}{18}P_2 = 0.$$

Hence we obtain

$$R^2 = -\frac{5^2}{4^2 27^3}P_2^3\left(40 + \frac{P_2}{P_1^2}\right).$$

The sign of R may be taken $+$, for the roots θ_1, θ_2, θ_3 all lie between $-a^2$ and $-c^2$, and if they are arranged in ascending order of numerical magnitude, r and t will be positive and s negative.

(ξ, η, ζ), Θ_1, Θ_2, Θ_3.

5. The harmonics of seventh degree can be determined in a similar manner. As, however, the expressions are somewhat longer to write, it will be sufficient to state the leading subsidiary results for one type in such a form that similar relations can be easily found for the other two by interchange of letters. The type chosen is $\zeta\Theta_1\Theta_2\Theta_3$.

$$\text{(A)}\quad 5(b^2+u)(c^2+u)+5(c^2+u)(a^2+u)+7(a^2+u)(b^2+u) = -14\mathrm{V}.$$

$$\text{(B)}\quad 5(s-r)(r-t)(t-s)\left[4(a^2+u)+4(b^2+u)+3(c^2+u)\right] = 4(a^2+u)(c^2+u)\mathrm{V}-7\mathrm{V}^2.$$

$$\text{(C)}\quad 4(a^2+u)(b^2+u)(c^2+u)+4\mathrm{V}\left[5(a^2+u)+5(b^2+u)+4(a^2+u)\right] = -5(s-r)(r-t)(t-s).$$

From these three results the equation for the sum of the roots and, with the aid of $r+s+t=0$, the equation for the difference of the roots can be readily formed, as in § 4.

6. To verify results proceed as follows:—Putting $p=0$, which is equivalent to making $a=b$, equation (1) or (2), for instance, one value of θ being thus $-a^2$, will give a second between $-b^2$ and $-c^2$, say $-x_1(c^2-a^2)$ and $-\frac{1}{2}x_1+\frac{1}{3}$ ought then to satisfy equation (13) with p zero. In like manner if we put $p=1$ or $b=c$, $\theta=-b^2$ will be one root and the other will lie between $-a^2$ and $-b^2$, say $-x_2(c^2-a^2)$, and $-\frac{1}{2}(1+x_2)+\frac{2}{3}$ ought then to satisfy (13) with p unity. By this means the accuracy of the results given in § 3 has been tested both when $p=0$ and $p=1$.

Further it appears that of the three members of any type of harmonic with two Θ factors one member has both θ roots between $-a^2$ and $-b^2$, another has both between $-b^2$ and $-c^2$, and the third has one root in one compartment and the other in the other. Again, with harmonics with three Θ factors one has all three values of θ between $-a^2$ and $-b^2$, a second all between $-b^2$ and $-c^2$ and the other two have respectively one in one compartment and two in the other.

[*Added March* 29. *Approximations.*—The solutions given above are applicable whether the ellipsoids are prolate or oblate, but in some of the physical problems in which the harmonics under consideration might be required, either a is nearly equal to b or b to c, and in those cases the exact expressions would be usefully replaced by series in ascending powers of p or $q\ (=1-p)$, p being applicable to a prolate and q to an oblate ellipsoid. Taking, for instance, the harmonic $\Theta_1\Theta_2$, we have already found the equation (11), which is suitable to the prolate form. The corresponding equation for the oblate is most readily obtained by writing

$$c^2-b^2,\quad c^2+u,\quad a^2+u,\quad b^2+u = (q,\ z,\ z-1,\ z-q)(c^2-a^2)$$

in the relation (7). We shall then obtain

$$49z^3-49(1+q)z^2+(12+37+12q^2)z-6(q+q^2)=0.$$

To solve this last in series proceeding in powers of q, observe that the part of the equation not involving q may be written $z(7z-4)(7z-3)$ and proceed by successive approximations. The roots will then be found to be

$$\left.\begin{array}{l} \frac{1}{2}q-\frac{1}{48}q^2-\frac{1}{96}q^3+\ldots, \\ \frac{3}{7}+\frac{1}{7}(2q-\frac{8}{3}q^2-\frac{4}{3}q^3+\ldots), \\ \frac{4}{7}+\frac{1}{7}(\frac{3}{2}q+\frac{45}{16}q^2+\frac{45}{32}q^3+\ldots), \end{array}\right\} \qquad (29)$$

and the corresponding values of y, for the differences,

$$\left.\begin{array}{l} \pm\sqrt{\frac{5}{168}}(q+\ldots), \\ \pm\sqrt{\frac{15}{343}}(1-\frac{1}{2}q+\frac{55}{72}q^2+\frac{55}{144}q^3+\ldots), \\ \pm\sqrt{\frac{8}{343}}(1-\frac{1}{2}q-\frac{127}{128}q^2-\frac{127}{256}q^3+\ldots). \end{array}\right\} \qquad (30)$$

If we compare the equation in z with that in x, it is clear that if we write p instead of q and change the signs of the series for z, we shall get the values of x and the corresponding differences.

This method applies to harmonics of the fifth degree, except that in the cases $\xi\Theta_1\Theta_2$ and $\zeta\Theta_1\Theta_2$, we shall not have the same simple relations between the equations in x and z.

When $p=q=\frac{1}{2}$ we reach the partition line between prolate and oblate ellipsoids, and at this particular point the expressions for the roots are simpler.]

On the Observations of Stars made in some British Stone Circles.—Second Note.

By Sir NORMAN LOCKYER, K.C.B., F.R.S., LL.D., Sc.D., Director Solar Physics Observatory.

(Received March 19,—Read March 29, 1906.)

In a preliminary note communicated to the Royal Society on March 15, 1905, I stated that I was attempting to continue here the researches in temple orientation carried on by myself in Egypt in 1891, by Mr. Penrose in Greece in 1892, and by both of us at Stonehenge in 1901.

I pointed out that from the observations I had made at the Hurlers and Stanton Drew, of which I gave an account, it seemed probable that the outstanding stones of our ancient monuments had been erected to assist astronomical observations.

Since the date of the preliminary note I have, in intervals of leisure, visited many of the British monuments, and friends have been good enough to make observations at others. I propose in the present note to state as briefly as possible the chief results I have obtained in cases where the enquiry has been complete enough to warrant definite conclusions being drawn. I have not in all cases been able to make a complete survey of the azimuths and the height of the sky-line of the existing monuments, and everywhere the destruction has been so serious that a complete story in any locality is out of the question.

Clock-Stars.

The practice so long employed in Egypt of determining time at night by the revolution of a star round the pole was followed in the British Circles.

This practice was to watch a first magnitude star, which I named a "clock-star,"* of such a declination that it just dipped below the northern horizon so as to be visible for almost the whole of its path.

One of the earliest temples in Egypt concerning which we have historical references to check the orientation results, was built to carry on these night observations at Denderah, lat. N. 26° 10′. The star observed was α Ursæ Majoris, decl. N. 58° 52′, passing 5° below the northern horizon; date (for horizon 1° high) about 4950 B.C., *i.e.*, in the times of the Shemsu Heru, before Mena, as is distinctly stated in the inscriptions.

After α Ursæ Majoris had become circumpolar in the latitude of Denderah, γ Draconis, which had ceased to be circumpolar, and so fulfilled the conditions to which I have referred, replaced it. Its declination was 58° 52′ N. about

* 'Dawn of Astronomy,' 1894, p. 343.

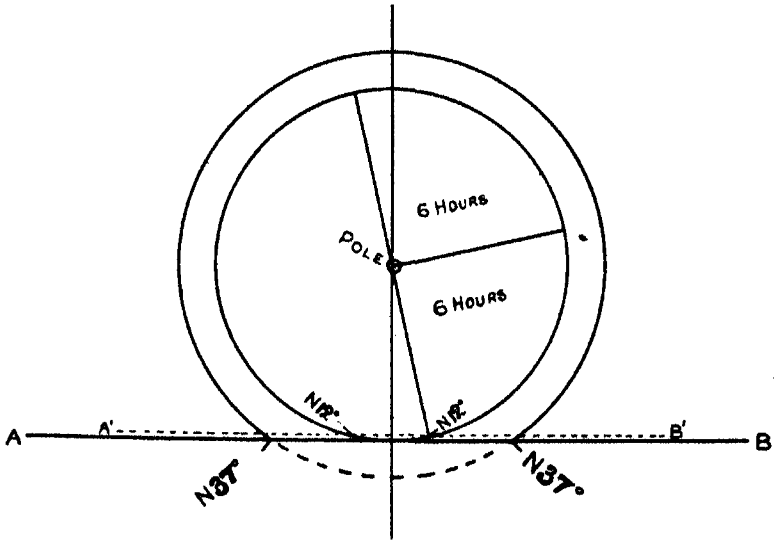
3100 B.C., and it therefore could have been watched rising in the axis prolonged of the old temple in the time of Pepi, who restored it then, and is stated to have deposited a copy of the old plan in a cavity in the new walls.

So far as the enquiry has gone, these clock-star observations were introduced into Britain about 2300 B.C. In my statement concerning them I will deal with the astronomical conditions for lat. 50° N., as it is in Cornwall that the evidence is most plentiful and conclusive.

In that latitude and at that time Arcturus, decl. N. 42°, was just circumpolar and therefore, with a sea horizon, neither rose nor set. Capella, decl. N. 31°, when northing, was 9° below the horizon, so that it rose and set in azimuths N. 37° E. and N. 37° W. respectively; it was therefore invisible for a long time and was an awkward clock-star in consequence.

Fig. 1 represents diagrammatically the conditions named, the circumpolar paths of Arcturus and Capella being shown by the smaller and the larger circle respectively. AB represents the actual sea horizon and A′B′ a locally raised horizon, whilst the dotted portion of the larger circle represents the non-visible part of Capella's apparent path.

FIG. 1.



What the British astronomer-priests did therefore in the majority of cases was to set up their temples in a locality where the N.E. horizon was high, so that Arcturus rose over it and was invisible for only a short time, as shown in the diagram by the raised horizon A′B′.

The two following lists contain the names and positions of monuments where Arcturus was used as a clock-star. In the first, the elevation of the

sky-line in each case has been actually measured, and the meaning and date of the alignment are therefore fairly trustworthy; but in the second list the elevations have been estimated from the differences of contour shown on the 1-inch Ordnance map, and the dates must be accepted as open to future revision.

Arcturus as a Clock-Star.

i.

Monument.	Position.		Alignment.	Az.	Hills.	Decl. N.	Date B.C.
	Lat. N.	Long. W.					
Tregeseal	° ′ ″ 50 7 50	° ′ ″ 5 39 20	Circ. to Carn Kenidjack	N. 12° 8′ E.	° ′ 4 0	° ′ 42 33	2330
			Circ. to barrow 800′ dist.	N. 20 8 E.	3 50	40 29	1970
The Hurlers* ...	50 31 0	4 27 20	S. circ. over cent. circle	N. 11 15 E.	3 24	41 38	2170
			Cent. circ. over N. circle	N. 14 18 E.	3 24	41 9	2090
			N. circ. over N.E. barrow	N. 18 44 E.	3 24	40 6	1900
Merrivale	50 33 15	4 2 30	Direction of smaller avenue	N. 24 25 E.	5 0	39 55	1860
Fernworthy ...	50 38 30	3 54 10	Direction of avenue	N. 13 0 E.	1 15	39 7	1720
			Second direction of avenue	N. 14 20 E.	1 15	38 51	1670
Stanton Drew...	51 22 0	2 34 30	Cent. of Gt. Circle to Quoit	N. 17 59 E.	2 33	38 38	1620
Fernworthy ...	50 38 30	3 54 10	Direction of avenue	N. 15 45 E.	1 15	38 34	1610
Merry Maidens	50 3 40	5 35 25	Circ. to stone in the road	N. 11 45 E.	0 12	38 27	1590
Stanton Drew...	51 22 0	2 34 30	S.W. circ. to cent. of Gt. Circ.	N. 19 51 E.	1 44	37 30	1420

* The dates here given for the Hurlers are earlier than those stated in the preliminary paper with an assumed sky-line. The actual elevation of the horizon has, in the meantime, been supplied by Captain Henderson. The alteration of the Stanton Drew date is not so great because the hills are lower.

ii.

Monument.	Position.		Alignment.	Az.	Hills.	Decl. N.	Date B.C.
	Lat. N.	Long. W.					
Trowlesworthy ...	° ′ ″ 50 27 30	° ′ ″ 4 0 20	Direction of primary avenue	N. 7° 0′ E.	° ′ 2 52	° ′ 41 24	2130
			Direction of final avenue	N. 12 0 E.	2 52	41 6	2080
Longstone (Tregeseal)	50 8 10	5 38 10	Longstone to Chûn Cromlech	N. 9 0 E.	1 43	40 39	2000
Lee Moor	50 26 30	3 59 40	Direction of avenue	N. 22 0 E.	2 28	38 17	1560

In some cases, for one reason or another, this arrangement was not carried out, and Capella, in spite of the objection I have stated, was used in the following circles:—

Capella as a Clock-Star.

Monument.	Position.		Alignment.	Az.	Hills.	Decl. N.	Date B.C.
	Lat. N.	Long. W.					
i.							
Boscawen-Un ...	50° 5′ 20″	5° 37′ 0″	Circ. to Stone Cross	N. 43° 15′ E.	2° 7′	29° 26′	2250
Merry Maidens...	50 3 40	5 35 25	Circ. over "The Pipers"	N. 38 26 E.	0 20	29 58	2160
ii.							
The Nine Maidens	50 28 20	4 54 30	Direction of Nine Maidens row	N. 28 0 E.	0 0	33 47	1480
Stripple Stones...	50 32 51	4 37 5	Centre to N.E. bastion	N. 26 0 E.	0 22	34 38	1320

At the Merry Maidens, however, with nearly a sea-horizon, when Arcturus ceased to be circumpolar, and rose and set in azimuth N. 11° 45′ E., it replaced Capella and was used as a clock-star after 1600 B.C.

The May-Year.

The first astronomical immigrants into Britain brought the May-year with them. This year is quartered by the sun's passage four times through 16° 20′ decl. N. and S., the Gregorian dates being May 6, August 8, November 8, and February 4.

There is evidence that this year was used in Babylon, Egypt, and afterwards in Greece. In the two former countries May was the harvest month, and thus became the chief month in the year. The dates were apt to vary slightly with the local harvest time. The earliest temple aligned to the sun at this festival seems to have been that of Ptah at Memphis, 5200 B.C. This date of the building of the temple is obtained by the evidence that the god Ptah represented the star Capella, as there is a Ptah temple at Thebes aligned on Capella and outside the solar limit.

There was also, in all probability, a similar temple at Annu (Heliopolis, lat. N. 30° 10′), but it has disappeared. The light of the sun fell along the axis when the sun had the decl. N. 11°, the Gregorian dates being April 18 and August 24.

Another May temple is that of Menu at Thebes (lat. N. 25°), date 3200 B.C., sun's decl. N. 15°, Gregorian date, May 1.

The researches of Mr. Penrose in Greece have provided us with temples

oriented to the May-year sun at Athens (including the Hecatompedon and older Erechtheum), Corinth, and Ægina.

The explorations of Sir H. Layard at Nineveh have shown that the temple in Sennacherib's palace was also oriented to the May sun.

Alignments in British monuments designed to mark the place of the sun's rising or setting on the quarter-days of the May-year have been found as follows:—

Monument.	Position.		May and August.		February and November.	
	Lat. N.	Long. W.	Rising.	Setting.	Rising.	Setting.
	° ′ ″	° ′ ″				
Merry Maidens	50 3 40	5 35 25	×	×		×
Boscawen-Un	50 5 20	5 37 0	×		×	?
Tregeseal	50 7 50	5 39 20	×		?	
Longstone (Tregeseal)	50 8 10	5 38 10	×			?
Down Tor	50 30 10	3 59 30	×			
Merrivale	50 33 15	4 2 30	×			
The Hurlers	50 31 0	4 27 20			×	?
Stonehenge	51 10 40	1 49 30	×	×		
Stanton Drew	51 22 0	2 34 30	×			
Stenness	59 0 10	3 13 40	×	×	×	×

It was the practice in ancient times for the astronomer-priests not only to watch the clock-stars during the night, but also other stars which rose or set about an hour before the sun so as to give warning of its approach on the days of the principal festivals.

Each clock-star, if it rose and set very near the north point, might be depended upon to herald the sunrise on *one* of the critical days of the year, but for the others other stars would require to be observed.

That this practice was fully employed in Britain is shown below:—

May Warnings.

Monument.	Star.	Date, or dates, B.C.
Stonehenge	Pleiades (R)	1950
Merry Maidens	Pleiades (R)	1930
	Antares (S)	1310
The Hurlers	Antares (S)	1720
	Pleiades (R)	1610
Merrivale	Pleiades (R)	1610
		1420
Boscawen-Un	Pleiades (R)	1480
Tregeseal	Pleiades (R)	1270
Stenness	Pleiades (R)	1230
Longstone (Tregeseal)	Pleiades (R)	1030

(R) = rising. (S) = setting.

August Warnings.—Sunrise at the August festival was heralded by the rising of Arcturus which, as we have seen, was also used as a clock-star.

The alignments and dates given in the Arcturus table therefrom hold good for August. At the Hurlers, where the hill over which Arcturus was observed fell away abruptly, we find Sirius supplanting Arcturus as the warning star for August in 1690 B.C.

November Warnings.—So far I have discovered no evidence that any star was employed to herald the November sun. There are two obvious reasons for this:—

In the first place, at the November festival the celebration took place at sun*set*, and the sun itself could be watched.

Secondly, the prevalent atmospheric conditions which obtain in Britain during November would not be conducive to the making of stellar observations *at the horizon;* and the people who built these temples only observed risings or settings.

February Warnings.—In just the same way that Arcturus served the double purpose of clock-star and herald for the August sun, so did Capella serve to warn the February sun in addition to its use as a clock-star. The alignments and dates given in the Capella table, will, therefore, hold good for its employment at the February quarter-day.

The Solstitial Year.

I have evidence that the observation and worship of the solstitial sun, such as was carried on in Egypt, at Karnak and possibly places of still greater antiquity,* was continued in other stone temples in Britain besides Stonehenge.

Although some of the alignments already found are in all probability solstitial, the variation of the sun's solstitial declination is so small that the most careful determination of their azimuths and angular elevations of the horizons must be made before the declinations and consequent dates can be arrived at.

Such a determination was made by Mr. Penrose and myself at Stonehenge in 1901 and reference to our paper† on the subject will show that, even after taking the greatest precautions, we were unable to fix the date of the monument with a smaller limit of error than 200 years.

Those monuments at which possible solstitial alignments have so far been found are given in the following table:—

* 'Dawn of Astronomy,' p. 78, London, 1894.
† 'Roy. Soc. Proc.,' vol. 69, pp. 137—147.

Monument.	Summer solstice.		Winter solstice.	
	Rising.	Setting.	Rising.	Setting.
Stonehenge	×			
Stanton Drew	×			
Stenness	×		×	×
Boscawen-Un	×			
Tregeseal	×			
Longstone (Tregeseal)				×

In several instances, as for example at the Boscawen-Un circle, there are two stones near to the solstitial sight-line, one of which can never have been used to indicate the solstitial line. Nearly the same thing occurs at Stonehenge where the isolated monolith, the Friar's Heel, is near, but to the east of the solstitial sight-line (*i.e.*, the avenue).

It seems probable that the solstice festival being of fundamental importance with the temple builders, they needed some *days* of warning instead of the hour or so provided by an heliacal rising or setting of a star. For this reason the stone was erected so that sunrise would take place in its direction some days before the solstice. In all the cases yet noted this stone is on the equator side, *i.e.*, to the E. of the true solstitial line and so would act as a warner.

The Equinoctial Year.

Only in one or two of the temples yet investigated has any evidence of an equinoctial worship been discovered. Even in these cases it is not conclusive, so for the present I leave this part of the question open.

My best thanks for assistance in the present enquiry are due to the following:—

To Colonel Duncan A. Johnston, R.E., C.B., late Director-General of the Ordnance Survey, and to Colonel R. C. Hellard, R.E., the present Director-General, I am indebted for the azimuths of the side-lines on various 25-inch maps and of several important sight-lines.

Mr. W. E. Rolston, F.R.A.S., one of the computers in this observatory, has calculated the declinations of the sun and stars corresponding to the azimuths determined, the consequent dates being taken from the tables prepared by Mr. J. N. Stockwell, Dr. W. J. S. Lockyer and Dr. O. Danckwortt.

In obtaining local particulars and measurements I have received invaluable assistance from Captain J. S. Henderson and Mr. Horton Bolitho

at the Hurlers, Professors Lloyd Morgan and Morrow and Mr. Dymond at Stanton Drew, and Messrs. H. Bolitho, H. Thomas and Captain Henderson in south-west Cornwall.

To Lord Falmouth and Mr. Wallis I am also under obligations, as they were good enough to assist my inquiries by allowing an opening to be made in a stone wall at the Merry Maidens to view the alignment to the Pipers.

On the Distribution of Radium in the Earth's Crust, and on the Earth's Internal Heat.

By the Hon. R. J. Strutt, F.R.S., Fellow of Trinity College, Cambridge.

(Received March 30,—Read April 5, 1906.)

CONTENTS.

§ 1.—*Introduction.*

Professor Rutherford* has given a calculation which suggests that there may be enough radium in the earth to account for the temperature gradient observed near the surface.

The question is of great interest from a cosmical point of view. For if we find that the earth's internal heat is due to radio-activity, and if we assume, as has been usual, that this heat is due to some vestiges of the cause operative in the sun and stars, it would follow that these latter are heated by radio-active changes also.

Professor Rutherford's calculation was based on some data given by Elster and Geitel on the amount of radium emanation which diffused out from a sample of clay. These data were obtained at a time when the quantitative determination of minute amounts of radium was not well understood, and are moreover inadequate to give any general idea of the average amount

* 'Radio-activity,' p. 494, 2nd Edition.

of radium in the earth's crust. I have, therefore, made an extensive investigation of the amount of radium in various representative rocks. This forms the subject of the present paper. The results are very surprising. Considerable detail will therefore be given, in order to enable readers to judge whether there is any probability of these results being substantially incorrect.

§ 2.—*Choice of Material.*

The earth's crust, which is alone accessible to us, consists of igneous rocks, and of sedimentary material which results from the action of geological changes upon these rocks. No doubt the average radium content of the original igneous rocks might be inferred fairly well from the examination of a large number of sedimentary ones. It is, however, much more satisfactory to examine the igneous materials directly, for then they can be classed among themselves as to their radium content.

I have examined a few sedimentary rocks, but do not attach much importance to them, and rely chiefly on results obtained with original igneous material for determining the average radium content of the earth's crust. The results with regard to sedimentary rocks will be given in a future paper. I hope also to determine which of the numerous minerals contained in igneous rocks carry the radium, a subject not touched in the present communication.

Meteorites have a special interest of their own. A few determinations have been made on them, and are incidentally included in this paper.

§ 3.—*Method of Determining Radium Content.*

Radium in the rocks was quantitatively determined by means of its emanation. A solution of the rock was stored till the emanation had accumulated. This latter was then extracted by boiling, and introduced into an electroscope. The increased rate of leak produced was a measure of the amount of radium present. This measure was made absolute by going through the same process with a uranium mineral of known radium content.

In order to make certain of extracting the emanation quantitatively, it is essential to decompose the rock completely by chemical agency. In the case of limestones and metallic meteorites, this can be effected by solution in hydrochloric acid, but in the case of siliceous materials, fusion with alkaline carbonate is necessary.

The standard procedure was as follows:—Fifty grammes of the rock was broken up in an iron mortar, and then finely ground in an agate one, until

it would pass through a sieve of 90 threads to the inch. This process could be carried out by an unskilled assistant in less than an hour, and ensured easy decomposition of the rock.

Two hundred and fifty grammes of a mixture of anhydrous sodium and potassium carbonates was melted in a large platinum basin. For this purpose the basin was surrounded with an extemporised furnace casing of asbestos millboard, and heated from below by means of a gas blowpipe. The blowpipe was supplied with air from an automatic blowing apparatus worked by water pressure. As soon as the carbonate was melted, the rock powder was thrown on to its surface in small portions at a time until it had all been added. The fusion was usually continued for about an hour after all effervescence was over.

The residue was then digested with hot water to dissolve out the alkaline silicates and carbonates. The portion insoluble in water was dissolved in hydrochloric acid. Some silica always separated from the acid solution, and was allowed to remain floating in it.

The two solutions, aqueous and acid, were set aside in separate flasks closed with indiarubber stoppers. Mixing them was avoided, because of the bulky and unmanageable precipitate of silica which would have been thrown down.

Decomposition with hydrofluoric acid has some advantages over the use of sodium carbonate. It requires, however, more minute pulverisation of the material, and is more costly in practice owing to difficulties of carriage and storage. After one or two trials its use was abandoned.

The two flasks containing the respective solutions were allowed to stand for some determinate number of days. A week was the minimum. More commonly a fortnight or three weeks was allowed. During this time, any radium contained in the rock was generating emanation. After three weeks the quantity has practically reached a maximum. The fraction of this maximum generated in any lesser period could be calculated from the equation for the rise of activity, originally given by Rutherford and Soddy.

I described a method for quantitatively extracting the emanation in 'Roy. Soc. Proc.,' A, vol. 76, 1905, p. 89. An improved modification of that method has been employed in the present investigation. The flask A (see figure) contains the solution. The accumulated emanation is at first partly dissolved, partly contained in the air which occupies the upper part of the flask. The flask is uncorked and attached to the lower end of the condenser B as rapidly as possible, so as to avoid loss of emanation. A is then boiled to expel radium emanation. The steam which issues is condensed in B and drops back. The air charged with emanation passes out

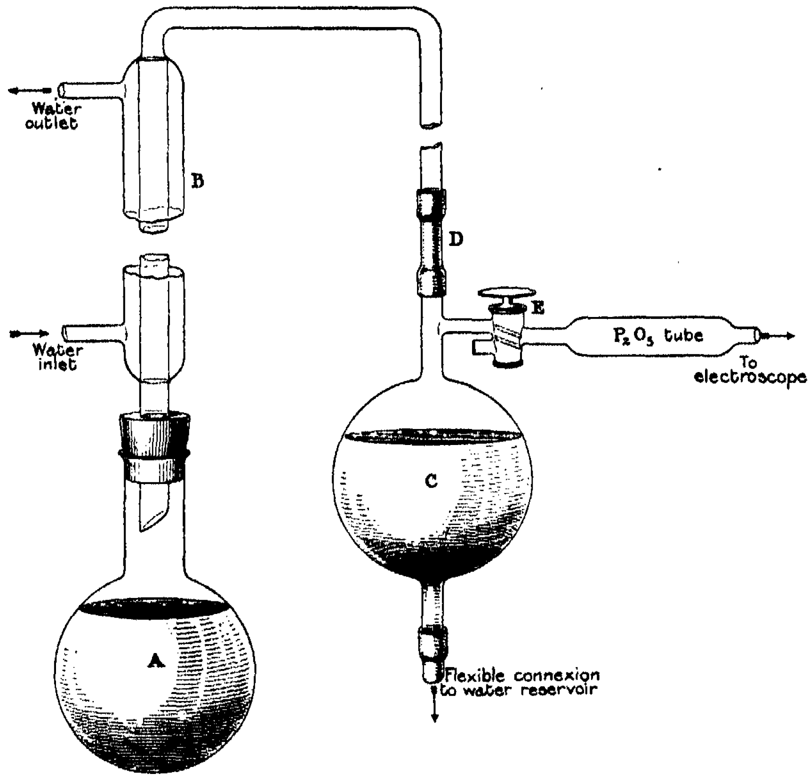


FIG. 1.

into the gasholder C, displacing the water which previously filled it. This boiling is continued for one hour. At the end of that time, the cooling water is run off from the jacket of B. Steam is allowed to pass so as to wash out all air and emanation from A and from the connecting tubes into C. The indiarubber connection at *d* is then nipped,* and the burner under the flask immediately withdrawn.

In this way the emanation is collected in the gasholder *c*. It merely remains to transfer it to the electroscope when cold. The latter is exhausted, and the emanation allowed to pass into it through the stop-cock E and a drying tube. Air is then admitted to the electroscope up to atmospheric pressure. After an interval of three hours, to allow the active deposit to form, the rate of leak is read.

The normal leak of the electroscope was repeatedly determined in the course of the investigation.

* To do this conveniently, it is very desirable to have the pinchcock attached to a firm support, so that it can be screwed up with one hand.

The following are the values in scale-divisions per hour, 24, 25·3, 24·1, 24, 23, 20·6, 21·7, 22·3, 22·5, 22·1. Mean, 23 nearly. These values were taken soon after exhaustion of the electroscope and admission of air.

If the instrument was left closed, the leak was found to have risen appreciably, after the lapse of a day. A similar effect has been noticed at the Cavendish Laboratory, and is, I believe, under investigation there. It does not come into question here, since time was never allowed for it to enter.

In work of this kind, when the effect to be looked for is small, it is most necessary to make certain that the emanation really comes from the material under investigation, and not from any extraneous source. I was deceived in this way in concluding that mercury gave off an emanation. In the present case, every precaution was taken.

The laboratory had never had radio-active materials introduced into it. Solutions of the reagents employed were separately tested for emanation, after they had stood closed for a fortnight, with the following results:—

	Rate of leak.
Sodium carbonate, 250 grammes	23·9
Potassium carbonate, 250 grammes	24·2
Hydrochloric acid, 500 c.c.	24·0
Water (Cambridge supply) 2000 c.c.	24·0

In none of these cases does the leak measurably exceed that normal to the electroscope. It may, therefore, be concluded that the reagents used to decompose the rocks are not responsible for the emanation obtained.

Cambridge tap water was used to make the solutions and to fill the gas-holder C (figure). This water originally contains dissolved emanation, as Professor J. J. Thomson has shown. It was therefore carefully boiled to expel this before use. The test given above was made on water which had already been boiled before setting it aside, and shows that no measurable quantity of emanation is generated by dissolved material when the original supply has been expelled. The (boiled) water in the gasholder was changed after each experiment.

Rutherford and Boltwood* have determined the radium content of uranium minerals in absolute measure. They find that the radium associated with 1 gramme of uranium is $7 \cdot 4 \times 10^{-7}$ gramme. I use this value rather than my own† because they tested the heat production of their standard radium, while I had no test for the purity of mine.

* 'Amer. Journ. Sci., 1905, p. 55.

† 'Roy. Soc. Proc.,' A, vol. 76, 1905, p. 88.

The uranium minerals used for standardisation were Torbernite (copper uranium phosphate) containing 60 per cent. U_3O_8 (= 51 per cent. uranium) and pitchblende, containing 73·5 per cent. U_3O_8 (= 62·4 per cent. uranium).

The rate of leak due to the emanation produced by a few milligrammes of each of these in *one day* was determined. From this the quantity of radium was deduced, which would in an *indefinite time* produce enough emanation to give a leak of one division per hour. This is the constant given in the last column of the following table.

Standardisation Experiments.

Mineral.	Percentage of uranium (metal).	Quantity, grammes.	Rate of leak* due to emanation accumulated in one day.	Constant deduced.
Torbernite	51·0	0·0076	214·0	$2·3 \times 10^{-12}$
" same sample......	51·0	0·0076	209·0	$2·36 \times 10^{-12}$
Torbernite	51·0	0·0046	116·5	$2·56 \times 10^{-12}$
" same sample......	51·0	0·0046	113·0	$2·64 \times 10^{-12}$
Pitchblende	62·4	0·0063	207·0	$2·41 \times 10^{-12}$
" same sample ...	62·4	0·0063	202·0	$2·45 \times 10^{-12}$

* Corrected for normal leak of the electroscope.

The mean value for the constant is $2·45 \times 10^{-12}$, or in other words a leak equal to one scale-division per hour represents $2·45 \times 10^{-12}$ gramme of radium in the sample examined, if the latter has had time to produce its maximum amount of emanation.

Two specimen determinations will now be given in detail, the first of granite from the Cape of Good Hope, the second of olivine rock from the Isle of Rum. These are respectively representative of high and low radium content.

Cape Granite, 50 grammes.—Solutions stood from March 21 to March 26, giving 60 per cent. of the maximum amount of emanation.

	Scale-div. per hour.	Corrected.
Emanation from acid solution	103	80·0
Emanation from alkaline solution ...	31	8·0

Total ...... 88·0 scale-div. per hour.

Thus the equilibrium amount of emanation would give 146 scale-divisions per hour, or per gramme of rock $\frac{146}{50} = 2·92$ scale-divisions per hour. Thus 1 gramme of rock contains $2·92 \times 2·45 \times 10^{-12}$ gramme or $7·15 \times 10^{-12}$ gramme radium.

Olivine Rock, Isle of Rum, 50 grammes.—Solutions stood from January 31 to February 19, giving practically the equilibrium amount of emanation.

	Scale-div. per hour.	Corrected.
Emanation from acid solution	34·0	11·0
Emanation from alkaline solution...	25·8	2·8

Total ...... 13·8 scale-div. per hour.

One gramme of rock would give 0·276 scale-division per hour, and contains $2{\cdot}45 \times 0{\cdot}276 \times 10^{-12} = 0{\cdot}676 \times 10^{-12}$ gramme radium.

This last example represents, as mentioned above, nearly the lowest radium content encountered among igneous rocks. It will be noticed that the leak produced by the emanation is, even in this unfavourable case, about half that normal to the electroscope, and is, therefore, quite well marked. It will be noticed also that the alkaline solution contains only a small proportion of the total radium present in the original rock.

§ 4.—*Results for Igneous Rocks.*

The results for igneous rocks will now be given in tabular form and in order of radium content (see next page).

It will be observed that, in general, rocks like granite, with a high percentage of silica, are richer in radium than basic rocks. The rule, however, is by no means invariable.

Uranium ores occur in several of the rocks examined; thus the granites Nos. 2 and 6 occur near the pitchblende bearing veins which were worked at Wheal Trenwith, St. Ives. The Norwegian syenite rocks, Nos. 3, 8, and 15, also contain local deposits of uranium bearing minerals. These various rocks are fairly rich in radium, but do not stand in a class by themselves.

Thus the concentration of radium in such deposits is extremely local, and cannot appreciably disturb any general conclusions as to the total amount of radium in the earth's crust.

Confirmatory of this conclusion is the fact that the temperature gradient at Wheal Trenwith, St. Ives, is quite normal.* Considerable quantities of pitchblende occur in this mine, but they are evidently insufficient to cause appreciable disturbance in the distribution of temperature.

* See Prestwich, 'Controverted Questions in Geology, p. 216.

No.	Name of rock.	Locality.	Density.	Radium per gramme, in grammes.	Radium per c.c., in grammes.
1	Granite	Rhodesia	2·63	$9·56\times10^{-12}$	$25·2\times10^{-12}$
2	"	Lamorna Quarry, Cornwall	2·62	$9·35\times10^{-12}$	$24·5\times10^{-12}$
3	Zircon syenite	Brevig, Norway	2·74	$9·30\times10^{-12}$	$25·5\times10^{-12}$
4	Granite	Rosemorran, Cornwall (?)	2·62	$8·43\times10^{-12}$	$22·1\times10^{-12}$
5	"	Cape of Good Hope	2·67	$7·15\times10^{-12}$	$19·1\times10^{-12}$
6	"	Knill's Monument, near Carbis Bay, St. Ives, Cornwall	2·61	$6·90\times10^{-12}$	$18·0\times10^{-12}$
7	"	Shap Fell, Westmoreland	2·65	$6·63\times10^{-12}$	$17·6\times10^{-12}$
8	Elæolite syenite	Laurdal, Norway	2·70	$4·88\times10^{-12}$	$13·2\times10^{-12}$
9	Granite	Haytor, Devonshire ?	2·61	$3·69\times10^{-12}$	$9·64\times10^{-12}$
10	Blue ground	Kimberley	3·06	$3·37\times10^{-12}$	$10·3\times10^{-12}$
11	Leucite basanite	Mt. Somma, Vesuvius	2·72	$3·33\times10^{-12}$	$9·07\times10^{-12}$
12	Hornblende granite	Assouan, Egypt	2·64	$2·45\times10^{-12}$	$6·47\times10^{-12}$
13	Pitchstone	Isle of Eigg	2·41	$2·06\times10^{-12}$	$4·97\times10^{-12}$
14	Hornblende diorite	Schriesheim, near Heidelberg	2·89	$1·98\times10^{-12}$	$5·73\times10^{-12}$
15	Augite syenite	Laurvig, Norway	2·73	$1·86\times10^{-12}$	$5·07\times10^{-12}$
16	Peridotite	Isle of Rum	3·15	$1·37\times10^{-12}$	$4·32\times10^{-12}$
17	Olivine basalt	Talisker Bay, Skye	2·89	$1·32\times10^{-12}$	$3·82\times10^{-12}$
18	Olivine euchrite	Isle of Rum	2·97	$1·28\times10^{-12}$	$3·80\times10^{-12}$
19	Basalt	Victoria Falls	2·75	$1·26\times10^{-12}$	$3·46\times10^{-12}$
20	Hornblende granite	Mt. Sorrel, Leicestershire	2·71	$1·25\times10^{-12}$	$3·38\times10^{-12}$
21	Dolerite	Isle of Canna	2·95	$1·24\times10^{-12}$	$3·65\times10^{-12}$
22	Greenstone	Carrick Dû, St. Ives	2·99	$1·14\times10^{-12}$	$3·41\times10^{-12}$
23	Basalt	Giants' Causeway, Antrim	2·80	$1·03\times10^{-12}$	$2·89\times10^{-12}$
24	Serpentine	Cadgwith, Lizard	2·60	$1·00\times10^{-12}$	$2·60\times10^{-12}$
25	Granite	Isle of Rum	2·61	$0·723\times10^{-12}$	$1·89\times10^{-12}$
26	Olivine rock	Isle of Rum	3·22	$0·676\times10^{-12}$	$2·18\times10^{-12}$
27	Dunite	L. Scaivig	3·34	$0·664\times10^{-12}$	$2·22\times10^{-12}$
28	Basalt	Ovifak, Disco Island, Greenland	3·01	$0·613\times10^{-12}$	$1·84\times10^{-12}$

§ 5.—*Meteorites.*

Determinations have been made on one stony meteorite, on three samples of meteoric iron, and on a sample of native iron from Ovifak, Disco Island, Greenland. The quantities available have been various, and are entered on the list of results. Where the radium is entered as 0, it is to be understood that no leak was obtained which could clearly be distinguished from that normal to the electroscope.

It will be observed that the stony meteorite contains about as much radium as those basic terrestrial rocks, which it resembles in general composition. No evidence was obtained of the presence of radium in iron meteorites. The Greenland iron contains a little radium. This was probably present in the siliceous material contained in this iron, which had been filtered off, decomposed by fusion and added to the main solution.

Material.	Locality.	Quantity taken.	Radium per gramme.
Stony meteorite	Dhurmsala	50 grammes	$1{\cdot}12 \times 10^{-12}$
Meteoric iron	Thunda	60 ,,	0
,, ,, ...	Augusta Co., Virginia	32 ,,	0
,, ,, ...	Santa Catarina	50 ,,	0
Native iron.................	Disco Island, Greenland	200 ,,	$0{\cdot}424 \times 10^{-12}$

§ 6.—*The Earth's Internal Heat.*

If q be the mean mass of radium per cubic centimetre in the earth, h the heat production of radium per gramme per second, then the total heat production must be, per second,

$$q \,.\, h \,.\, \tfrac{4}{3}\pi R^3.$$

If k be the thermal conductivity of the surface rocks, then the total outflow of heat per second will be

$$4\pi R^2 k \,.\, \left(\frac{d\theta}{dr}\right)_R,$$

where $\left(\frac{d\theta}{dr}\right)_R$ is the temperature gradient near the surface, as observed experimentally.

If the earth is in a thermally steady state, then these expressions will be equal, *i.e.*,

$$4\pi R^2 k \left(\frac{d\theta}{dr}\right)_R = \tfrac{4}{3}\pi R^3 q h,$$

or

$$q = \frac{3k}{hR}\left(\frac{d\theta}{dr}\right)_R.$$

For k I take the value 0·0041.* For the temperature gradient 1° F. in 42·4† feet, or, in C.G.S. system, $4{\cdot}3 \times 10^{-4}$.

One gramme of radium *bromide* produces 100 calories per hour. This gives for h the value $4{\cdot}75 \times 10^{-2}$.

R, the earth's radius, $= 6{\cdot}38 \times 10^8$.

Thus

$$q = \frac{3 \times 4{\cdot}1 \times 10^{-3} \times 4{\cdot}3 \times 10^{-4}}{4{\cdot}75 \times 10^{-2} \times 6{\cdot}38 \times 10^8} = 1{\cdot}75 \times 10^{-13}.$$

If, therefore, we assume the earth to be in thermal equilibrium, then, even if the whole of the internal heat is due to radium, the mean quantity per cubic centimetre cannot much exceed $1{\cdot}75 \times 10^{-13}$ gramme per cubic

* See Prestwich, *loc. cit.*

† *Loc. cit.*

centimetre, always supposing that the heat production of radium is not materially diminished under the conditions prevailing inside the earth.

Rutherford,* taking somewhat different values for the constants involved, gave the value $2{\cdot}6 \times 10^{-13}$ *radium bromide*, equivalent to $1{\cdot}52 \times 10^{-13}$ gramme *radium* per cubic centimetre.

It will be observed that all the igneous rocks examined, without exception, contain far more radium per cubic centimetre than this. The poorest of all, Greenland basalt, contains more than 10 times as much; an average rock something like 50 or 60 times.

The question must be faced: Why has not the earth a temperature gradient far larger than that observed?

The calculation given above assumes,—

(1) That the earth is in thermal equilibrium, *i.e.*, that the amount of heat which escapes per second is equal to the supply produced in that time.

(2) That no other source of internal heat than radium exists.

(3) That a gramme of radium produces as much heat inside the earth as at the surface.

As to the first assumption, to suppose that the earth is cooling only aggravates the difficulty. To assume that it is getting hotter is an explanation not likely to be regarded with favour. I have not yet considered it quantitatively.

As to the second assumption, there can be little doubt that a quantity of uranium proportional to the amount of radium exists in the rocks.† Moreover, a trace of thorium is not improbably present. These sources of heat are not likely to be important, as compared with radium. There is, too, the possibility of the radio-activity of ordinary materials. If, however, a heating effect from them is assumed, of the order of magnitude to be expected from the ionisation they give, a temperature gradient would result something like 1000 times larger than that observed. I think this argument is conclusive against the theory that they have a genuine radio-activity of this order of magnitude.‡

There remains the third assumption, stated above. This cannot be passed over so lightly as the others, but will be more conveniently discussed later. I shall suppose for the moment that it is justified, and that the earth cannot contain more on the average than $1{\cdot}75 \times 10^{-13}$ gramme of radium per cubic centimetre.

For surface rocks the experiments show that 5×10^{-12} gramme per cubic

* *Loc. cit.*

† Too little to be detected in the ordinary course of analysis.

‡ See 'Nature,' December 21, 1905.

centimetre is a representative value. This is, if anything, an understatement. Thus not more than about 1/30 of the earth's volume can consist of material similar to that encountered on the surface. This gives a depth of about 45 miles for the rocky crust, assuming the total absence of radio-active material within.

I shall next consider the distribution of temperature in a crust of this thickness. The curvature of so thin a crust is relatively small, and may, without appreciable sacrifice of accuracy, be disregarded. Let x be the depth at any point, measured from the outer surface, q the quantity of radium per cubic centimetre, h the heat developed in one second by 1 gramme of radium, θ the temperature at the depth x, and k the thermal conductivity.

Then the equation of conduction is

$$\frac{d^2\theta}{dx^2} = \frac{-qh}{k},$$

which gives by integration

$$\theta = \frac{-qh}{2k}(x^2+ax+b),$$

where a and b are constants.

If the surface of the earth be assumed to be at 0° C., then $\theta = 0$ when $x = 0$.

If t be the thickness of the crust, $d\theta/dx = 0$, when $x = t$. This is clear, since the temperature must be a maximum at the inner boundary of the crust. The interior core, free from radium, must be at a uniform temperature throughout.

Substituting these values we get

$$b = 0 \quad \text{and} \quad a = -2t.$$

Hence

$$\theta = \frac{qhx}{2k}(2t-x).$$

Substituting the numerical values adopted in this paper,

$$\theta = \frac{5 \times 10^{-12} \times 4{\cdot}75 \times 10^{-2} x\,(2 \times 7{\cdot}25 \times 10^{6} - x)}{2 \times 4{\cdot}1 \times 10^{-3}} = 2{\cdot}90 \times 10^{-11} x\,(1{\cdot}45 \times 10^{7} - x).$$

The curve appended shows this distribution of temperature graphically.

The maximum temperature at the bottom of the crust will be where $x = 7{\cdot}25 \times 10^6$. This gives $\theta = 2{\cdot}90 \times 10^{-11} \times (7{\cdot}25)^2 \times 10^{12} = 1530°$, a temperature considerably below the melting point of platinum.*

* No doubt the assumption of constant conductivity at all temperatures is an unsatisfactory feature in this calculation. It is difficult to know what other assumption to make, however. Some experiments of Lord Kelvin and Mr. Erskine Murray ('Roy. Soc. Proc.,' vol. 58, p. 162) seem to indicate a diminution of conductivity with temperature.

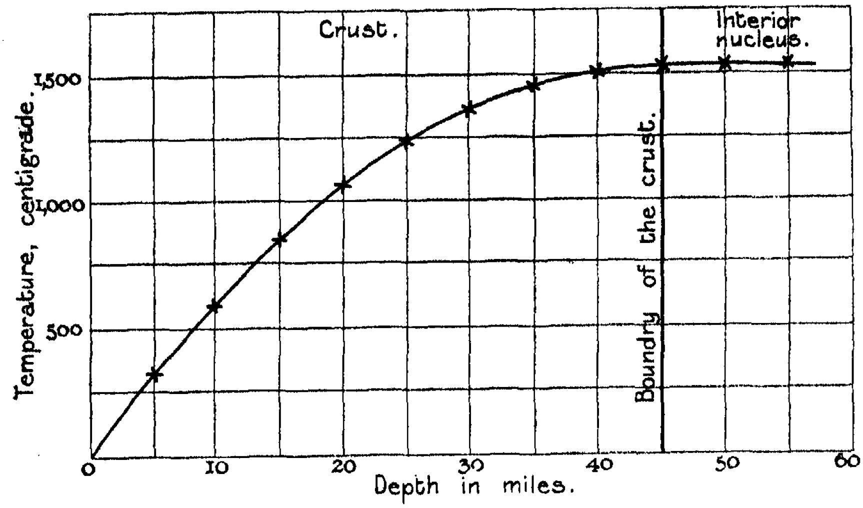


FIG. 2.—Calculated distribution of temperature in the earth's interior.

This result has been obtained on the provisional assumption that the heat production of radium is the same throughout the earth's crust as under surface conditions. In justification of this, a paper by Mr. W. Makower* may be referred to. The activity of radium emanation and its products are shown to be substantially the same at temperatures of 1200° as at the ordinary temperature, though evidence was obtained of a slight change of activity in one of the products. Thus there is no reason at present to think that notable change of activity sets in before 1500° is reached. I wish to express myself with some reserve on this subject. Further experiments might conceivably show a rapid loss of activity as this temperature was approached. In that case the conclusions here drawn as to the earth's internal condition would require modification.

I was inclined at first to think it incredible that the earth's crust could have so small a thickness as 45 miles, and was therefore much interested to hear that Professor Milne† had come to a substantially identical conclusion, from a study of the velocity of propagation of earthquakes through the earth's interior. He gives 30 miles as the thickness. This is quite

On the other hand, Mr. C. H. Lees found the thermal conductivity of window glass to increase with temperature; and at high temperatures rock magmas must approach the quality of glass; indeed, they sometimes even retain that quality on cooling (obsidian and pitchstone). The mean thermal conductivity of rock cannot be much *more* than that assumed, for otherwise the internal temperature would not be high enough to produce volcanic phenomena.

* 'Roy. Soc. Proc.,' A, vol. 77, p. 241.

† Bakerian Lecture, 1906.

consistent with my data, if rocks like granite, rich in radium, are assumed to be somewhat more abundant than I have supposed, in taking the value 5×10^{-12} gramme radium per cubic centimetre as representative.

Professor Milne expresses the opinion that a fairly abrupt transition occurs at a depth of 30 miles, and that the material below that depth is fairly uniform throughout the globe. This is entirely in agreement with the view put forward above with regard to the earth's interior.

The chemical nature of the interior is a difficult problem. It can scarcely consist mainly of iron, as has been very commonly supposed, from the analogy of meteorites. Meteoric iron is remarkably free from radium, as shown above, and in this respect answers the requirements well enough. But if the stony exterior of the earth is but a small fraction of the whole volume, it cannot have much influence on the mean density, which should be nearly equal to that of the core. The density of the earth (5·5) is much less than the density of iron (7·7).

§ 7.—*Internal Heat of the Moon.*

The data of this paper have an interesting application to the moon. What we can observe of the moon's surface suggests that it consists of rock like that on the earth. The moon is believed to have originally separated from the earth's surface, and should, therefore, consist of the same material as the latter. Moreover, the density of the moon (3·5) does not differ much from that of rock. It seems reasonable to conclude from these facts that the moon consists almost entirely of rock similar to that of the earth's crust.

On this view, the temperature gradient of the moon should be very great in comparison with that of the earth. The material of the moon is taken to be some 30 times richer in radium than the (mean) material of the earth. Her volume is about one-fiftieth that of the earth. Thus the total heat production in the moon would be about half of that in the earth. This heat has to flow out through about one-sixteenth the area of the earth surface. Thus the temperature gradient at the moon's surface should be eight times greater than at the earth's.

In addition to this, gravity is very much less on the moon. We may conclude that the conditions which prevail there are far more favourable to manifestation of the internal heat by volcanic upheaval. This fully explains why volcanic features are so much more prominent on the moon than on the earth.

It has generally been supposed that the lunar craters are extinct. But

that view seems to rest chiefly on an *a priori* conviction that the moon has no internal heat. As Professor W. H. Pickering has pointed out, all those observers who have made a special study of the moon have believed in the reality of changes occurring there.*

§ 8.—*Summary of Conclusions.*

1. Radium can easily be detected in all igneous rocks. Granites, as a rule, contain most radium, basic rocks the least.

2. This distribution of radium is uniform enough to enable a fair estimate to be made of the total quantity in each mile of depth of the crust.

3. The result indicates that the crust cannot be much more than 45 miles deep, for otherwise the outflow of heat would be greater than is observed to be the case. The interior must consist of some totally different material. This agrees entirely with Professor Milne's conclusion drawn from a study of the velocity of propagation of earthquake shocks through the interior.

4. The moon probably consists for the most part of rock, and if so, its internal temperature must be far greater than that of the earth. This explains the great development of volcanoes on the moon.

5. Iron meteorites contain little, if any, radium. Stony ones contain about as much as the terrestrial rocks which they resemble.

In conclusion, I must thank Mr. A. Harker, Mr. Clement Reid, and Mr. A. Hutchinson for their kindness in providing me with various specimens of rock, and for information on geological matters.

* 'Nature,' January 5, 1905.

On the Dilatational Stability of the Earth.

By Lord Rayleigh, O.M., Pres. R.S.

(Received March 14,—Read March 29,—Revised April 16, 1906.)

The theory of elastic solids usually proceeds upon the assumption that the body is initially in a state of ease, free from stress and strain. Displacements from this condition, due to given forces, or vibrations about it, are then investigated, and they are subject to the limitation that Hooke's law shall be applicable throughout and that the strain shall everywhere be small. When we come to the case of the earth, supposed to be displaced from a state of ease by the mutual gravitation of its parts, these limits are transgressed; and several writers* who have adopted this point of view have indicated the obstacles which inevitably present themselves. In his interesting paper† Professor Jeans, in order to attain mathematical definiteness, goes the length of introducing forces to counteract the self-gravitation: "That is to say, we must artificially annul gravitation in the equilibrium configuration, so that this equilibrium configuration may be completely unstressed, and each element of matter be in its normal state." How wide a departure from actuality is here implied will be understood if we reflect that under such forces the interior of the earth would probably be as mobile as water.

It appears to me that a satisfactory treatment of these problems must start from the condition of the earth as actually stressed by its self-gravitation, and that the difficulties to be faced in following such a course may not be so great as has been supposed. The stress, which is so enormous as to transcend all ordinary experience, is of the nature of a purely hydrostatic pressure, and as to this surely there can be no serious difficulty. After great compression the response to further compressing stress is admittedly less than at first, but there is no reason to doubt that the reaction is purely elastic and that the material preserves its integrity. At this point it may be well to remark, in passing, upon the confusion often met with in geological and engineering writings arising from the failure to distinguish between a one-dimensional and a three-dimensional, or hydrostatic, pressure. When rock or cast iron is said to be *crushed* by such and such a pressure, it is the

* See, for example, Love, 'Theory of Elasticity,' § 127; Chree, 'Phil. Mag., vol. 32, p. 233, 1891; Jeans, 'Phil. Trans.,' A, vol. 201, p. 157, 1903.

† *Loc. cit.*, p. 161.

former kind of pressure which is, or ought to be, meant. There is no evidence of crushing under purely hydrostatic pressure, however great.

Not only is the integrity of a body unimpaired by hydrostatic pressure, but there is reason to think that the superaddition of such a pressure may preserve a body from rupture under stresses that would otherwise be fatal. FitzGerald raises this question in a review* of Hertz's 'Miscellaneous Papers.' He writes: "In considering the cracking of a material like glass, Hertz seems to think its cracking will depend only on the tension; that it will crack where the tension exceeds a certain limit. He does not seem to consider whether it might not crack by shearing with hardly any tension. It is doubtful whether a material in which there were sufficient general compression to prevent any tension at all, would crack. Rocks seem capable of being bent about and distorted to almost any extent without cracking, and this might very well be expected if they were at a sufficient depth under other rocks to prevent their parts being under tension. It is an interesting question whether a piece of glass could be bent without breaking if it were strained at the bottom of a sufficiently deep ocean. On the other hand, there seems very little doubt that the parts of a body might slide past one another under the action of a shear, and would certainly crack unless there were a sufficiently great compressional stress to prevent the crack; and that consequently a body might crack, even though the tensions were not by themselves sufficiently great to cause separation, and might crack where the shear was greatest, and not where the tensions were greatest."

When we reflect that pieces of lead may be made to unite under pressure when the surfaces are clean, and upon what is implied when insufficiently lubricated journals, or slabs of glass under polish, *seize*, we may well doubt whether it is possible to disintegrate a material at all when subjected to enormous hydrostatic pressure. In the words of Dr. Chree†: "The conditions under which the deep-seated materials of the earth exist are fundamentally different from those we are familiar with at the surface. The enormous pressure, and the presumably high temperature, very likely combine to produce a state to which the terms solid, viscous, liquid, as we understand them, are alike inapplicable."

A study of the mechanical operations of coining and of stamping (in recent years, I believe, much developed) would probably throw light upon this question. We know that rod or tube may be "squirted" from hot (but solid) lead. Is the obstacle to a similar treatment of harder material purely practical? In the laboratory I have experimented upon jellies of various

* 'Nature,' November 5, 1896; 'Scientific Writings,' p. 433.

† 'Phil. Mag.,' vol. 43, p. 173, 1897.

degrees of stiffness, on the principle of suiting the material to the appliances rather than the appliances to the material. In the simplest arrangement a leaden bullet is imbedded in jelly contained in a strong glass tube which the bullet somewhat nearly fits. Although the tube stand vertical for several days, there is no appreciable descent. But if by numerous longitudinal impacts against a suitable pad the inertia of the bullet be brought into play, movements through several inches may be obtained. Here, although the deformations are very violent, there is no rupture visible, either before or behind the bullet.

When an elastic body is slightly displaced from the condition of ease, the potential energy (V) is expressed by terms involving the squares and products of the displacements. If, however, we suppose given finite forces to be constantly imposed, so that the initial condition is one of strain, the case is somewhat, though not essentially, altered. It may be convenient to make a statement, once for all, in terms of generalised co-ordinates. If under the action of the forces Φ, Θ, ... the co-ordinates assume the values ϕ, θ, ... we have in terms of the potential energy of strain V,

$$\Phi = \frac{dV}{d\phi}, \qquad \Theta = \frac{dV}{d\theta}, \text{ etc.} \tag{1}$$

If the forces permanently imposed be distinguished by the suffix (0), they are connected with the corresponding values of the co-ordinates, ϕ_0, etc., by the equations

$$\Phi_0 = \frac{dV}{d\phi_0}, \qquad \Theta_0 = \frac{dV}{d\theta_0}, \text{ etc.} \tag{2}$$

This strained condition is now to be regarded as initial, and displacements from it are denoted by ascribing to the co-ordinates the slightly altered values $\phi_0 + \delta\phi$, $\theta_0 + \delta\theta$, etc. For the potential energy of strain we have

$$V - V_0 = \frac{dV}{d\phi_0}\delta\phi + \frac{dV}{d\theta_0}\delta\theta + \tfrac{1}{2}\frac{d^2V}{d\phi_0^2}(\delta\phi)^2 + \frac{d^2V}{d\phi_0\,d\theta_0}\delta\phi\,.\,\delta\theta + \ldots, \tag{3}$$

which is of the *first* order of the small quantities $\delta\phi$, etc. But $V - V_0$ is not now the whole potential energy. In addition to the potential energy of strain we have to include that of the steadily imposed forces, represented by the terms

$$-\Phi_0\,\delta\phi - \Theta_0\,\delta\theta - \ldots. \tag{4}$$

The whole potential energy is thus

$$V - V_0 - \Phi_0\,\delta\phi - \Theta_0\,\delta\theta - \ldots = \tfrac{1}{2}\frac{d^2V}{d\phi_0^2}(\delta\phi)^2 + \frac{d^2V}{d\phi_0 d\theta_0}\delta\phi.\delta\theta + \tfrac{1}{2}\frac{d^2V}{d\theta_0^2}(\delta\theta)^2 + \ldots, \tag{5}$$

regard being paid to (2). The total potential energy, as given by (5), is now

of the second order in $\delta\phi$, etc., as is obviously required by the circumstance that the strained condition ϕ_0, etc., is one of equilibrium under the proposed forces. The coefficients of stability are $d^2V/d\phi_0^2$, $d^2V/d\phi_0 d\theta_0$, etc., and they may differ finitely from the values which obtained previously to the application of the forces Φ_0, etc.

As an example having an immediate bearing upon the matter in hand, let us consider the case of a uniform non-gravitating body originally in a state of ease. If a small hydrostatic pressure Φ act upon it, the volume ϕ changes proportionally, and the ratio gives the "compressibility" of the body in this condition. Under the action of a finite pressure Φ_0 the volume may be greatly altered, especially if the body be gaseous, but the new condition is still one of equilibrium and may be regarded as initial. The compressibility now may be quite different from before, but it may be treated in the same way as depending upon the small *change* of volume $\delta\phi$ accompanying the imposition of a small *additional* pressure $\delta\Phi$.

To those who, while accepting the usual elastic theory for bodies in a state of ease, repudiate the application to bodies subject to great hydrostatic pressure, I would suggest that liquids and solids, as we know them, are not really free from stress. In virtue of cohesional forces, there is every reason to believe, the interior of a drop of water is under pressure not insignificant even in comparison with those prevailing inside the earth, and the same may be said of a piece of steel.

The conclusion that I draw is that the usual equations may be applied to matter in a state of stress, provided that we allow for altered values of the elasticities. In general, these elasticities will not only vary from point to point, but be æolotropic in character. If, however, we suppose that the body is naturally isotropic, and that the imposed stress is everywhere merely a hydrostatic pressure, so that by pure expansion a state of ease could be attained, the case is much simpler and probably suffices for an approximate view of the condition of the earth. But although the initial state is one free of shear, we are not to conclude that the rigidity is the same as it would be without the imposed pressure. On the contrary, there is much reason to think that the rigidity would be increased. If there is any analogy to be found in a pile of mutually repellant hard spheres, it will follow that an infinite pressure will entail infinite rigidity as well as infinite incompressibility.

In the original draft of this paper I had supposed that it would be possible upon these lines to find another and a more practical basis for Professor Jeans' analysis. A correspondence with Professor Love* has, however, convinced me that this hope is destined to disappointment, and the remainder

* To whom I am indebted also for other corrections.

of the paper loses accordingly much of the interest which at first I felt for it. In Professor Jeans' theory, if Δ be the dilatation, so that the altered density is $\rho(1-\Delta)$, U the radial outward displacement, E the potential of a volume-distribution of density $\rho\Delta$, and a surface-distribution of density $-\rho U$, the displacements ξ, η, ζ are subject to

$$\rho\frac{d^2\xi}{dt^2} = (\lambda+\mu)\frac{d\Delta}{dx} + \mu\nabla^2\xi - \rho\frac{dE}{dx}, \tag{6}$$

and two similar equations relating to η, ζ. In (6) λ and μ are the elastic constants of Lamé's notation, and they relate to displacements from the *compressed* initial condition. From equations (6) we obtain, as usual,

$$\rho\frac{d^2\Delta}{dt^2} = (\lambda+2\mu)\nabla^2\Delta - \rho\nabla^2 E; \tag{7}$$

and by Poisson's equation

$$\nabla^2 E = -4\pi\gamma\rho\Delta, \tag{8}$$

γ being the constant of gravitation. Thus

$$\rho\frac{d^2\Delta}{dt^2} = (\lambda+2\mu)\nabla^2\Delta + 4\pi\gamma\rho^2\Delta, \tag{9}$$

which is Professor Jeans' equation.*

The solution of these equations is developed by Professor Jeans with the view of determining at what point instability sets in. Attention is concentrated mainly upon the solution of (9) expressed by a spherical function of order *one*, as being that which bears upon the question of the evolution of the moon.

I had intended merely to indicate a somewhat simpler treatment, following more closely the notation and method of Lamb's memoir, "On the Vibrations of an Elastic Sphere;"† but as the results so obtained do not agree with those of Jeans, it appears necessary to set forth the argument in fuller detail, so as to facilitate criticism.

If in (9) we assume that Δ is proportional to $\cos pt$, we get

$$(\nabla^2 + h^2)\Delta = 0, \tag{10}$$

where

$$h^2 = \frac{\rho(p^2+4\pi\gamma\rho)}{(\lambda+2\mu)}; \tag{11}$$

and the solution of (10), subject to the condition of finiteness at the centre, is

$$\Delta = (hr)^{-\frac{1}{2}} J_{n+\frac{1}{2}}(hr) \,.\, S_n \,.\, \cos pt, \tag{12}$$

J being the symbol of Bessel's functions, and S_n a spherical surface function

* *Loc. cit.*, p. 162.

† 'Proceedings London Mathematical Society,' vol. 13, p. 192, 1882.

of order n. As is well known, $J_{n+\frac{1}{2}}$ is expressible in finite terms; in the case of $n = 0$, $(hr)^{\frac{1}{2}}J_{\frac{1}{2}}(hr)$ may be replaced in (12) by $\sin hr/hr$, a constant factor being disregarded.

Before going further it may be well to consider the particular case of a *fluid* for which $\mu = 0$. Here the solution for Δ already given suffices to solve the problem, and the condition of no pressure at the surface ($r = a$) gives at once

$$J_{n+\frac{1}{2}}(ha) = 0, \tag{13}$$

which with (11) determines p^2 in terms γ, ρ, a and the elastic constant λ. The criterion of stability follows by setting $p = 0$. In the case of $n = 0$, where the displacements are symmetrical, $ha = m\pi$, m being an integer; and we see that equilibrium is unstable for symmetrical displacements if

$$a^2\rho^2\gamma > \tfrac{1}{4}\pi\lambda. \tag{14}$$

In general, by (8) and (10)

$$\nabla^2 E = \frac{4\pi\gamma\rho}{h^2}\nabla^2\Delta, \tag{15}$$

so that

$$E = \frac{4\pi\rho\gamma}{h^2}\Delta + e, \tag{16}$$

where e satisfies throughout the sphere

$$\nabla^2 e = 0. \tag{17}$$

Substituting the value of E in (6), we get with regard to (11)

$$(\nabla^2 + k^2)\,\xi = \left(1 - \frac{k^2}{h^2}\right)\frac{d\Delta}{dx} + \frac{\rho}{\mu}\frac{de}{dx}, \tag{18}$$

where

$$k^2 = p^2\rho/\mu. \tag{19}$$

Equation (18) and its companions may be treated as in Lamb's classical paper. A solution is

$$\xi = -\frac{1}{h^2}\frac{d\Delta}{dx} + \frac{1}{p^2}\frac{de}{dx}, \quad \text{etc.}, \tag{20}$$

where Δ satisfies (10). In virtue of (17) these values satisfy the relation

$$\frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz} = \Delta;$$

and the solution may be completed by the addition of terms u, v, w, satisfying $(\nabla^2 + k^2)\,u = 0$, etc., as well as the relation

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

Professor Lamb gives the general values of u, v, w. For our present

purpose, and with limitation to one order of spherical harmonics, it suffices to take

$$u = \psi_{n-1}(kr)\frac{d\phi_n}{dx} - \frac{n}{n+1}\,\frac{k^2 r^{2n+3}}{2n+1\,.\,2n+3}\,\psi_{n+1}(kr)\,\frac{d}{dx}\,\frac{\phi_n}{r^{2n+1}}, \tag{21}$$

and two similar equations, where ϕ_n is a solid harmonic of degree n; $r = \sqrt{(x^2+y^2+z^2)}$; and ψ_n is defined by the equation

$$\psi_n(\theta) = 1 - \frac{\theta^2}{2\,.\,2n+3} + \frac{\theta^4}{2\,.\,4\,.\,2n+3\,.\,2n+5} - \dots \tag{22}$$

Save as to a constant multiplier $\theta^n\psi_n(\theta)$ is identical with $\theta^{-\frac{1}{2}}J_{n+\frac{1}{2}}(\theta)$, as employed in (12). ψ_n is thus associated with *solid* in place of *surface* harmonics. The function possesses the following properties

$$\psi_n'(\theta) = -\frac{\theta}{2n+3}\,\psi_{n+1}(\theta); \tag{23}$$

$$\psi_n(\theta) + \frac{\theta}{2n+1}\,\psi_n'(\theta) = \psi_{n-1}(\theta); \tag{24}$$

$$\psi_n(\theta) - \psi_{n-1}(\theta) = \frac{\theta^2}{2n+1\,.\,2n+3}\,\psi_{n+1}(\theta). \tag{25}$$

A formula in spherical harmonics frequently required is

$$x\phi_n = \frac{r^2}{2n+1}\left\{\frac{d\phi_n}{dx} - r^{2n+1}\frac{d}{dx}\,\frac{\phi_n}{r^{2n+1}}\right\}. \tag{26}$$

The term of the nth order in Δ is thus

$$\Delta_n = \psi_n(hr)\,.\,\omega_n \tag{27}$$

and corresponding thereto

$$\xi = -\frac{1}{h^2}\,\frac{d\Delta_n}{dx} + \frac{1}{p^2}\,\frac{de_n}{dx} + u, \tag{28}$$

where u is defined as above, and e_n, as well as ϕ_n and ω_n, is a solid harmonic of degree n.

The formation of the boundary conditions to be satisfied at the free surface of the sphere $(r = a)$ proceeds almost exactly as in Lamb's investigation (p. 199), the only difference arising from the fact that h has now a different value. The first of the three symmetrical surface conditions may be written

$$\frac{\lambda}{\mu}\,x\Delta_n + \left(r\frac{d}{dr} - 1\right)\xi + \frac{d}{dx}(x\xi + y\eta + z\zeta) = 0. \tag{29}$$

The terms in (29) depending on the parts of ξ, η, ζ which involve Δ_n are found to be

$$A_n\frac{d\omega_n}{dx} + B_n\frac{d}{dx}\,\frac{\omega_n}{r^{2n+1}}, \tag{30}$$

where

$$A_n = \frac{\lambda+2\mu}{\mu}\frac{a^2\psi_n(ha)}{2n+1} - \frac{2n-2}{h^2}\psi_{n-1}(ha); \tag{31}$$

$$B_n = -\frac{\lambda+2\mu}{\mu}\frac{a^{2n+3}}{2n+1}\psi_n(ha) + \frac{2(n+2)\,a^{2n+3}}{2n+1\,.\,2n+3}\psi_{n+1}(ha). \tag{32}$$

In like manner Lamb finds for the terms in (29) arising from u, v, w,

$$C_n\frac{d\phi_n}{dx} + D_n\frac{d}{dx}\frac{\phi_n}{r^{2n+1}}, \tag{33}$$

where

$$C_n = -\left\{\frac{k^2a^2}{2n+1}\psi_n(ka) - 2(n-1)\psi_{n-1}(ka)\right\}; \tag{34}$$

$$D_n = -\frac{n}{n+1}\frac{k^2a^{2n+3}}{2n+1}\left\{\psi_n(ka) + \frac{2(n+2)}{k^2a^2}ka\,\psi_n'(ka)\right\}. \tag{35}$$

We have now further an additional part arising from e_n, which, it should be observed, makes no contribution to Δ_n. In this

$$\left(r\frac{d}{dr}-1\right)\frac{de_n}{dx} = (n-2)\frac{de_n}{dx},$$

$$x\frac{de_n}{dx} + y\frac{de_n}{dy} + z\frac{de_n}{dz} = n\,e_n;$$

so that the additional part is

$$\frac{2n-2}{p^2}\frac{de_n}{dx}. \tag{36}$$

The two most important cases where $n = 0$ and $n = 1$ are also especially simple, in that (36) disappears. It will be convenient to consider them first. When $n = 0$, u, v, w vanish: also, since e_0 is constant, (28) reduces to

$$\xi = -\frac{1}{h^2}\frac{d\Delta_0}{dx}, \quad \eta = -\frac{1}{h^2}\frac{d\Delta_0}{dy}, \quad \zeta = -\frac{1}{h^2}\frac{d\Delta_0}{dz}, \tag{37}$$

where Δ_0 is proportional to $\psi_0(hr)$. The motion is everywhere purely radial. Exactly as in Lamb's investigation of vibrations without gravity, the expression (30) reduces to

$$B_0\frac{d}{dx}\frac{\omega_0}{r},$$

where ω_0 is a constant, so that the surface conditions yield simply $B_0 = 0$, or from (32)

$$\frac{\lambda+2\mu}{\mu}\psi_0(ha) = \tfrac{4}{3}\psi_1(ha). \tag{38}$$

Writing θ for ha, and for ψ_0 and ψ_1 $(= -3\theta^{-1}\psi_0')$ their values, we get

$$\tan\theta = \frac{4\theta}{4-\theta^2\dfrac{\lambda+2\mu}{\mu}}. \tag{39}$$

Except for a slight difference of notation, this is the same as Lamb's equation, and his results are therefore available. They are expressed by means of Poisson's elastic constant σ, and they exhibit ha/π as dependent on σ and on the order of the root. To adapt them it is only necessary to remember that h^2, as given by (11), has here a different value from that which obtains when there is no gravitation ($\gamma = 0$). On the other hand, although γ be finite, ha/π may still be equated to T_1/τ, where T_1 is the time occupied by a plane wave of longitudinal vibration in traversing a space equal to the diameter of the sphere, and τ denotes the time of complete oscillation. The following are the smallest values of ha/π corresponding to selected values of σ, as given by Lamb:—

$\sigma = 0.$	$\sigma = \frac{1}{4}.$	$\sigma = \frac{3}{10}.$	$\sigma = \frac{1}{3}.$
0·6626	0·8160	0·8500	0·8733

For example, if $\sigma = \frac{1}{4}$ (Poisson's value), the criterion of stability is

$$\frac{4\pi\gamma\rho^2 a^2}{\lambda + 2\mu} < (0{\cdot}8160)^2.$$

If $\sigma = \frac{1}{2}$, the material is incompressible, and motion of the kind now under contemplation is excluded.

When $n = 1$, (36) again vanishes, though for a different reason from before.* The form of the solution is accordingly the same as if there were no gravitation. We have from (31), (32)

$$A_1 = \frac{\lambda + 2\mu}{\mu} \frac{a^2 \psi_1(ha)}{3}, \tag{40}$$

$$B_1 = -\frac{\lambda + 2\mu}{\mu} \frac{a^5 \psi_1(ha)}{3} + \frac{2a^5 \psi_2(ha)}{5}; \tag{41}$$

and from (34), (35) omitting some common factors which have no effect,

$$C_1 = \psi_1(ka); \tag{42}$$

$$D_1 = \frac{a^3}{2}\left\{\psi_1(ka) + \frac{6}{k^2a^2} ka\psi_1'(ka)\right\} = \frac{a^3}{2}\left\{\psi_1(ka) - \tfrac{6}{5}\psi_2(ka)\right\}. \tag{43}$$

The surface conditions (29) are of the form

$$A_1 \frac{d\omega_1}{dx} + B_1 \frac{d}{dx} \frac{\omega_1}{r^3} + C_1 \frac{d\phi_1}{dx} + D_1 \frac{d}{dx} \frac{\phi_1}{r^3} = 0,$$

and, as Professor Lamb shows, they require that

$$A\omega_1 + C_1\phi_1 = 0; \qquad B_1\omega_1 + D_1\phi_1 = 0. \tag{44}$$

It follows that ϕ_1 and ω_1 must be of the same form, and also that

$$B_1/A_1 = D_1/C_1, \tag{45}$$

* The terms de_1/dx, etc., in (28) denote in this case a uniform displacement, as of a rigid body, and naturally contribute nothing to the surface condition.

in which the values of A_1, B_1, C_1, D_1 are to be substituted from (40), (41), (42), (43). We find

$$\frac{4\mu}{\lambda+2\mu}\frac{\psi_2(ha)}{\psi_1(ha)} = 5-2\frac{\psi_2(ka)}{\psi_1(ka)}; \tag{46}$$

or if, in accordance with (23), we replace $\psi_2(\theta)$ by $-5\,\theta^{-1}\psi_1'(\theta)$,

$$-\frac{4\mu}{\lambda+2\mu}\frac{\psi_1'(ha)}{ha\,.\,\psi_1(ha)} = 1+\frac{2\psi_1'(ka)}{ka\,.\,\psi_1(ka)}. \tag{47}$$

Except for the different value of h, this agrees with Professor Lamb's equation (87).

In equation (46) there is no limitation upon the value of p. If to find the criterion of stability we put p or k equal to zero,

$$\psi_2(ka) = \psi_1(ka) = 1,$$

and the equation reduces to

$$\psi_2(ha) = \frac{3(\lambda+2\mu)}{4\mu}\psi_1(ha). \tag{48}$$

The equation may also be written in terms of the Bessel functions. The relation between J and ψ is

$$\begin{aligned}
J_{\frac{1}{2}}(x)\times\sqrt{(\tfrac{1}{2}\pi x)} &= x\,\psi_0(x) &&= \sin x,\\
J_{\frac{3}{2}}(x)\times\sqrt{(\tfrac{1}{2}\pi x)} &= \tfrac{1}{3}x^2\psi_1(x) &&= x^{-1}\sin x-\cos x,\\
J_{\frac{5}{2}}(x)\times\sqrt{(\tfrac{1}{2}\pi x)} &= \tfrac{1}{15}x^3\psi_2(x) &&= (3x^{-2}-1)\sin x-3x^{-1}\cos x;
\end{aligned}$$

so that in terms of J's (48) becomes

$$\frac{\lambda+2\mu}{\mu}J_{\frac{3}{2}}(x) = \frac{20}{3x}J_{\frac{5}{2}}(x); \tag{49}$$

or, if we introduce the circular functions,

$$\frac{(3-x^2)\tan x-3x}{x^2\tan x-x^3} = \tfrac{3}{20}\frac{\lambda+2\mu}{\mu}. \tag{50}$$

Unfortunately (49) does not agree with the result given by Professor Jeans. In his notation, when $n=1$,

$$y_1\text{ (from 59)} = -\tfrac{13}{15}\frac{\rho a^2}{\mu}, \qquad y_2\text{ (from 60)} = +\tfrac{1}{30}\frac{\rho a^2}{\mu};$$

and (54), (57), (58) give

$$\frac{\lambda+2\mu}{\mu}J_{\frac{3}{2}}(x) = \frac{156}{25x}J_{\frac{5}{2}}(x). \tag{51}$$

The comparison of processes is rendered difficult by the occurrence of several errors (possibly misprints) in Professor Jeans' paper. Thus (33) does not seem correct, and (41), (42) do not follow from (38), (39). Starting from Professor Jeans' equations just mentioned and making use of his (30),

(43), (44), (45), (48), (49), I have obtained a result in harmony with my equation (49).

From (50) it is easy to calculate the value of λ/μ corresponding to any value of x. When $\mu = 0$, $\tan x = x$, of which the first root is

$$x = 1\cdot4303\pi = 4\cdot493.$$

This gives an angle of $77\frac{1}{2}^\circ$ ($+180^\circ$). Calculating for angles of 60°, 50°, 40°, we find

λ/μ	∞	3·840	2·056	1·221
x	4·493	4·189	4·014	3·840

It seems that the value of x is not very sensitive to variation of μ, and for such values of the ratio of λ/μ as are likely to occur, especially under high pressure, we might almost content ourselves with the fluid solution ($\mu = 0$).

The simplicity of the cases so far considered, viz., $n = 0$ and $n = 1$, depends upon our having escaped the necessity of determining the value of e_n. For values of n greater than unity this function remains in the equations, which now demand a more elaborate treatment. From (20), (21)

$$\xi = -\frac{1}{h^2}\frac{d\Delta_n}{dx} + \frac{1}{p^2}\frac{de_n}{dx} + u, \tag{52}$$

in which the second term becomes infinite when $p = 0$. In order to balance this, ϕ_n in (21) must be made infinite of the order p^{-2} or k^{-2}. Thus writing $k^{-2}\Phi_n = \phi_n$, we have

$$\frac{1}{p^2}\frac{de_n}{dx} + u = \frac{\rho}{\mu k^2}\frac{de_n}{dx} + \frac{1}{k^2}\left\{1 - \frac{k^2r^2}{2\,.\,2n+1}\right\}\frac{d\Phi_n}{dx}$$
$$-\frac{n}{n+1}\,\frac{r^{2n+3}}{2n+1\,.\,2n+3}\left\{1 - \frac{k^2r^2}{2\,.\,2n+5} + \ldots\right\}\frac{d}{dx}\frac{\Phi_n}{r^{2n+1}}.$$

Thus, as in the theory of differential equations with equal roots, we have when $k^2 = 0$,

$$\frac{1}{p^2}\frac{de_n}{dx} + u = \frac{df_n}{dx} + \frac{\rho}{\mu}\,\frac{r^2}{2\,.\,2n+1}\,\frac{de_n}{dx} + \frac{\rho}{\mu}\,\frac{n}{n+1}\,\frac{r^{2n+3}}{2n+1\,.\,2n+3}\,\frac{d}{dx}\,\frac{e_n}{r^{2n+1}}. \tag{53}$$

with two similar equations, f_n denoting again a solid harmonic of degree n. From (52), (53) we find for the radial displacement

$$\text{U} = \frac{x\xi + y\eta + z\zeta}{r} = -\frac{1}{h^2}\frac{d\Delta_n}{dr} + \frac{nf_n}{r} + \frac{\rho}{\mu}\,\frac{n\,r\,e_n}{2\,.\,2n+3}$$
$$= \frac{\omega_n}{h}\left\{-\frac{n}{ha}\psi_n(ha) + \frac{ha}{2n+3}\psi_{n+1}(ha)\right\} + \frac{nf_n}{a} + \frac{\rho}{\mu}\,\frac{n\,a\,e_n}{2\,.\,2n+3} \tag{54}$$

at the surface, where $r = a$.

The boundary condition (29) requires a parallel treatment. The terms depending on Δ_n remain as in (30), (31), (32). From (34), (35), we get as appropriate for the present purpose

$$C_n = (2n-2)\left\{1-\frac{k^2a^2}{2\,.\,2n+1}\right\}-\frac{k^2a^2}{2n+1}=2n-2-\frac{n\,k^2a^2}{2n+1},$$

$$D_n = \frac{n\,k^2a^{2n+3}}{n+1\,.\,2n+1\,.\,2n+3}.$$

Equations (33), (36) now give, Φ being written as before for $k^2\phi$,

$$(2n-2)\left\{\frac{\rho}{\mu k^2}\frac{de_n}{dx}+\frac{1}{k^2}\frac{d\Phi_n}{dx}\right\}-\frac{na^2}{2n+1}\frac{d\Phi_n}{dx}+\frac{n\,a^{2n+3}}{n+1\,.\,2n+1\,.\,2n+3}\frac{d}{dx}\frac{\Phi_n}{r^{2n+1}}, \tag{55}$$

which, when k^2 is made to vanish, is to be replaced by

$$(2n-2)\frac{df_n}{dx}+\frac{\rho}{\mu}\frac{na^2}{2n+1}\frac{de_n}{dx}-\frac{\rho}{\mu}\frac{na^{2n+3}}{n+1\,.\,2n+1\,.\,2n+3}\frac{d}{dx}\frac{e_n}{r^{2n+1}}. \tag{56}$$

This is additional to (30). The equations to be satisfied at the surface are thus

$$A_n\omega_n+(2n-2)f_n+\frac{\rho}{\mu}\frac{na^2}{2n+1}e_n = 0. \tag{57}$$

$$B_n\omega_n-\frac{\rho}{\mu}\frac{na^{2n+3}}{n+1\,.\,2n+1\,.\,2n+3}e_n = 0. \tag{58}$$

When $n = 1$, f_n disappears, and the final condition is found by eliminating the ratio $\omega_n : e_n$ from (57), (58). This would conduct us again to the results already arrived at for that case. In general we require another equation connecting e_n with ω_n and f_n.

For this purpose we must recur to the definition (16) of e_n and of E. A calculation is made by Jeans on the basis of the expression (12) of Δ by means of Bessel's functions. We have at the surface

$$e_n = \frac{4\pi\rho\gamma a}{2n+1}\left\{-U-(n+\tfrac{1}{2})\,h^{-1}(ha)^{-\frac{1}{2}}J_{n+\frac{1}{2}}(ha)\,.\,S_n-h^{-1}(ha)^{-\frac{1}{2}}J_n{}'_{+\frac{1}{2}}(ha)\,.\,S_n\right\}. \tag{59*}$$

In order to express this in our present notation (by means of ψ's), we see by comparison of (12) and (27) that

$$J_{n+\frac{1}{2}}(ha)\,.\,S_n = (ha)^{\frac{1}{2}}\psi_n(ha)\,.\,\omega_n, \tag{60}$$

so that with use of (24)

$$e_n = \frac{4\pi\rho\gamma a}{2n+1}\left\{-U-\frac{2n+1}{h^2a}\psi_{n-1}(ha)\,.\,\omega_n\right\}. \tag{61}$$

* In Professor Jeans' equations (5), (23) the sign of U is positive, but this appears to be an error.

Eliminating U between this and (54), we find

$$f_n = \frac{\rho a^2 e_n}{\mu}\left\{-\frac{(2n+1)\mu}{n(\lambda+2\mu)h^2a^2}-\frac{1}{2\,.\,2n+3}\right\}$$
$$+\frac{\omega_n}{h^2}\left\{-\frac{2n+1}{n}\psi_{n-1}+\psi_n-\frac{h^2a^2}{n\,.\,2n+3}\psi_{n+1}\right\}. \qquad (62)$$

The substitution for f_n in (57) now gives

$$\omega_n\left[A_n+\frac{2n-2}{h^2}\left\{-\frac{2n+1}{n}\psi_{n-1}+\psi_n-\frac{h^2a^2}{n\,.\,2n+3}\psi_{n+1}\right\}\right]$$
$$+\frac{\rho a^2 e_n}{\mu}\left[-\frac{(2n-2)(2n+1)\mu}{n(\lambda+2\mu)h^2a^2}-\frac{2n-2}{2\,.\,2n+3}+\frac{n}{2n+1}\right]=0. \qquad (63)$$

This equation and (58) determine two values of $\omega_n : e_n$, and the elimination of this ratio gives the required final result. We will write (63) for brevity as

$$Fa^2\omega_n+G\,(\rho a^2/\mu)\,e_n = 0, \qquad (64)$$

where by (31), and reduction with the aid of (25),

$$F=\frac{\lambda+2\mu}{\mu}\,\frac{\psi_n}{2n+1}+\frac{2n-2}{h^2a^2}\left\{-\frac{2n+1}{n}\psi_n+\frac{h^2a^2}{2n+1\,.\,2n+3}\psi_{n+1}\right\}, \qquad (65)$$

$$G=-\frac{(2n-2)(2n+1)\mu}{n(\lambda+2\mu)h^2a^2}-\frac{2n-2}{2\,.\,2n+3}+\frac{n}{2n+1}. \qquad (66)$$

Similarly, if (58) be written

$$Ha^2\omega_n+K\,(\rho a^2/\mu)\,e_n = 0, \qquad (67)$$

we may take

$$H=-\frac{\lambda+2\mu}{\mu}\,\frac{\psi_n}{2n+1}+\frac{2(n+2)}{2n+1\,.\,2n+3}\psi_{n+1} \qquad (68)$$

$$K=-\frac{n}{n+1\,.\,2n+1\,.\,2n+3}; \qquad (69)$$

and the final result is

$$FK-GH=0, \qquad (70)$$

giving the ratio $\psi_{n+1}(ha) : \psi_n(ha)$ in terms of n, ha, and $(\lambda+2\mu)/\mu$.

In applying results of calculation based upon the assumption of a uniform compressibility to the case of the earth where the variation is likely to be very considerable, we must have regard to the character of the function (12) by which the dilatation is expressed. When $n = 1$ or a greater number, (12) vanishes at the centre and (when $\mu = 0$) at the surface. The values to be ascribed to the elasticities are those proper to an intermediate position, such as half-way between the centre and the surface. For a more complete treatment we might calculate the balance of the elastic and gravitational potential energies on the basis of a displacement still following the same law

as has been found to apply to a uniform sphere. In accordance with a general principle the result, so calculated, will be correct as far as the first powers of the variations from uniformity.

Another question, interesting to geologists, upon which our results have a bearing is as to the effect of denudation in altering the surface level. The immediate effect of the removal of material is, of course, to lower the level, but if the material removed is heavy and the substratum very compressible, the springing up of the foundation may more than neutralize the first effect and leave the new surface higher than the old one. So far as I am aware discussions have been based upon the elastic quality merely of the interior without regard to self-gravitation; but, as is easy to see, if the condition be one approaching instability, the effect of a pressure applied to the surface may be immensely increased.

On the Specific Heat of, Heat Flow from, and other Phenomena of, the Working Fluid in the Cylinder of the Internal Combustion Engine.

By Dugald Clerk, M.Inst.C.E.

(Communicated by the Hon. C. A. Parsons, C.B., F.R.S. Received February 24,—Read March 15, 1906.)

The present investigation was undertaken with the object of determining the specific heat of, and heat-flow from, the highly heated products of combustion which constitute the working fluid within the cylinder of an internal combustion engine, by a method which permitted direct observations to be made upon an actual charge taken into the engine in the ordinary operations of its cycle.

The method of experiment is very simple, and the writer believes it to be novel. It consists in subjecting the whole of the highly heated products of the combustion of a gaseous charge to alternate compression and expansion within the engine cylinder while cooling proceeds, and observing by the indicator the successive pressure and temperature-falls from revolution to revolution, together with the temperature and pressure rise and fall due to alternate compression and expansion. The engine is set to run at any given speed, and at the desired moment after the charge of gas and air has been drawn in, compressed, and ignited, the exhaust valve and charge inlet valves are prevented from opening, so that when the piston reaches the termination of its power stroke, the exhaust gases are retained within the cylinder, and the piston compresses them to the minimum volume, expands them again to the maximum volume, and so compresses and expands during the desired number of strokes.

Fig. 1 is an indicator diagram so obtained, and it will be noted that, in addition to the usual power diagram, a series of lines are traced which show the gradual cooling of the hot contents of the cylinder through the enclosing walls. The mode of dealing with these observations will be apparent from the following considerations:—

If a gas be compressed without gain or loss of heat from volume V_0 to V_1, and temperature rises from T_0 to T_1, so that the work done upon the gas is W, then the mean specific heat C_v of the gas per unit volume at 0° and 760 mm. at constant volume between the temperatures is

$$C_v = \frac{W}{\psi_0 (T_1 - T_0)},$$

where ψ_0 is a constant depending on the quantity of gas in the cylinder.

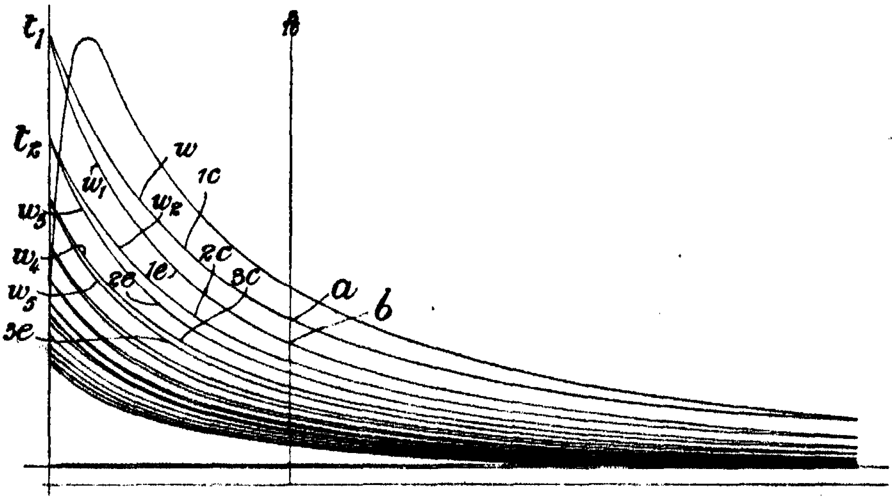


FIG. 1.

It will be found more convenient to consider the specific heat per cylinder full of gas; if this specific heat value be S,

$$S = C_v \psi_0 = \frac{W}{T_1 - T_0}. \tag{1}$$

If different gases could be sufficiently compressed adiabatically, and the work of compression accurately measured, the value of S could be obtained for any given temperature range. In actual apparatus, heat exchanges of course take place between the gas and its enclosing walls, so that this method cannot be experimentally applied without means of determining the law of heat flow to and from the gas.

Assume, however, that in the case of an expanding gas the temperature-fall due to heat-flow through the walls is known, and its value for a given case is θ_m, while the temperature-fall during the given expansion is $t_1 - t_{01}$; if the total work done by the expansion is W_m, then the mean specific heat S for the temperature range t_1 to t_{01} is

$$S = \frac{W_m}{(t_1 - t_{01}) - \theta_m}, \tag{2}$$

That is, by deducting the temperature-fall due to heat-flow from the total temperature-fall, as shown by the expanding line, there is at once obtained the temperature-fall which is caused by the gas doing work W_m upon the piston. It is therefore only necessary to determine the temperature-fall due to heat loss on any given expansion line to be enabled to at once calculate the mean specific heat of the gas for the temperature limits.

The law of heat-loss cannot be ascertained by any consideration of

compression or expansion curves separately, unless the specific heat values of the gas be known.

By subjecting a gas, however, to alternate compression and expansion, at a relatively rapid rate, the law of heat-flow and its absolute value can be ascertained, and hence the mean specific heat determined, whether it be constant or variable throughout the temperature range.

Assume a gas at a higher temperature than its enclosing walls to be alternately compressed from volume V_0 to V_1, and expanding from V_1 to V_0, as illustrated in fig. 2. The successive pressures at the volume V_0 are indicated at the points A, C, E, G, and I, while those at volume V_1 are B, D, F, H and J. The gas is thus compressed from A to B; C to D; E to F; G to H; and I to J; and it is expanded from B to C; D to E; F to G; and H to I. Assume that the gas is cooling but remains above the temperature of the walls during these compressions and expansions.

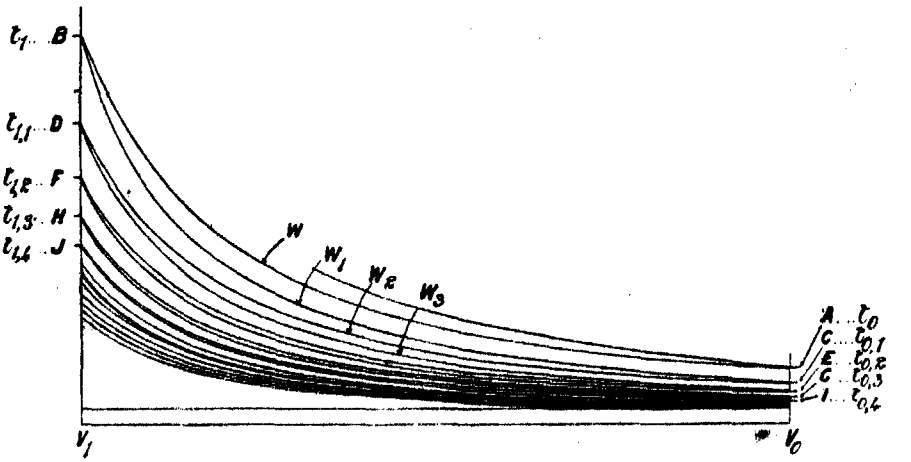


Fig. 2.

During the first compression, work is done upon the gas equal to W; during the first expansion, B to C, work is done by the gas upon the piston equal to W_1, and the pressure has fallen from the point A to the point C, that is, the gas has been compressed from the volume V_0 to V_1, and expanded back again to the same volume, while heat is being lost to the sides of the cylinder; work also has been done upon the gas, and has passed away as heat, because W is greater than W_1. If the absolute temperatures at A, C, E, G, and I be t_0, t_{01}, t_{02}, t_{03}, and t_{04}, then the total or true temperature-fall due to heat-loss to the walls in the double operation is

$$g_0 = (t_0 - t_{01}) + \frac{W - W_1}{S}, \qquad (3)$$

S being mean specific heat at constant volume in work units for the temperature range between A and C.

For the successive double operations between C, E, G and I, the true temperature-falls due to heat-loss through the walls are obtained in the same way. Taking temperature-falls between these points as g_{01}, g_{02}, g_{03}, etc.,

$$g_{01} = (t_{01}-t_{02})+\frac{W_2-W_3}{S_1}, \quad \text{etc.,} \tag{4}$$

that is, the true temperature-falls g_0, g_{01}, g_{02}, g_{03}, etc., are greater than the apparent falls (t_0-t_{01}), $(t_{01}-t_{02})$, etc., by temperature-fall equivalents of the respective work-areas $W-W_1$; W_2-W_3; etc.

During the first expansion from B to C work is done by the gas upon the piston equal to W_1, and the second compression, C to D, does work upon the gas W_2, and so on. Meantime the pressure has fallen from the point B to the point D; that is, the gas has been expanded from volume V_1 to V_0, and compressed back again from V_0 to V_1, while heat is being lost to the enclosing walls. Work, however, in this case has been done by the gas upon the piston, because W_1 is greater than W_2. At the minimum volume V_1 the successive points B, D, F, H and J indicate temperature-falls due not only to heat-loss through the walls, but also due to some work done upon the piston. If the absolute temperatures at B, D, F, H and J be taken as t_1, t_{11}, t_{12}, t_{13}, t_{14}, then the total or true temperature-falls due to heat-loss to the walls in the double operation of expansion and compression are

$$g_1 = (t_1-t_{11})-\frac{W_1-W_2}{S'}, \tag{5}$$

S′ being the mean specific heat at constant volume for the temperature range between B and D.

For the successive double operations between D, F, H and J, the temperature-falls due to heat-loss through the walls are obtained in the same way. Taking temperature-falls between those points as g_{11}, g_{12}, g_{13}, etc.,

$$g_{11} = (t_{11}-t_{12})-\frac{W_3-W_4}{S_1'}, \quad \text{etc.,} \tag{6}$$

that is, the true temperature-falls g_1, g_{11}, etc., are less than the apparent falls $(t_1 - t_{11})$, $(t_{11} - t_{12})$, etc., by the temperature equivalents of the respective work-areas $(W_1 - W_2)$, $(W_3 - W_4)$, etc. To calculate the specific heat from the expansion lines BC, DE, FG and HI, it is necessary to know the work-areas W_1, W_3, W_5 and W_7, as also the temperature-falls, θ_n, due to heat flow through the enclosing walls. All these values are given by direct readings from the diagrams, except the latter, θ_n, and to determine this it is necessary to convert the differences between the work-areas concerned into their

temperature-fall equivalents. For this purpose the values S, S_1, S′, S_1', are required; that is, the specific heats must be known before an accurate cooling curve can be prepared. This can be readily done with sufficient accuracy with two approximations. For a first approximation assume that (t_1-t_{11}), $(t_{11}-t_{12})$, etc., correctly represent the temperature-falls due to heat-flow through the walls; then construct a curve showing temperature-drop per revolution, shown in black line at fig. 3, which is taken from Card No. 22.*

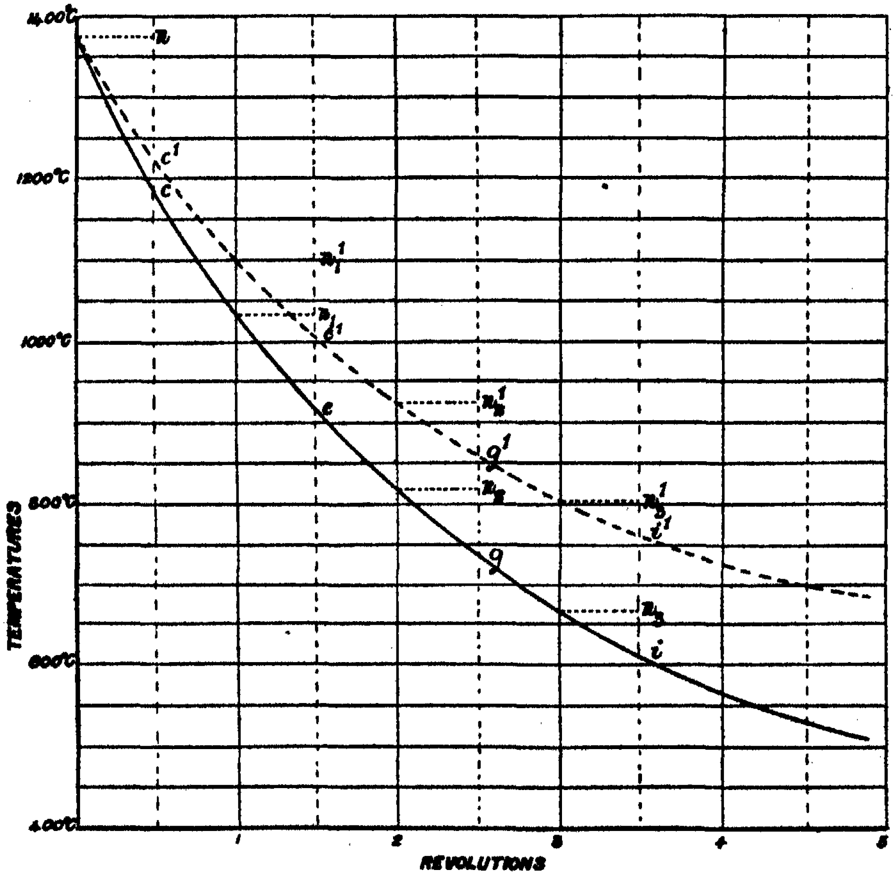


Fig. 3.

From this curve it will be seen that the temperature-drop assumed entirely due to heat-flow in one expansion stroke BC is equal to the ordinate *cn*; let

* The original enlarged indicator diagram cards are preserved for reference at the Royal Society.

this be g_n, then

$$S_n = \frac{W_1}{(t_1-t_{01})-g_n}. \tag{7}$$

Values similar to S_n are calculated from the losses on DE, FG, etc., then these values are used to calculate g_n, g_{n1}, g_{n2}, etc., the temperature-fall equivalents of the areas (W_1-W_2); (W_3-W_4); (W_5-W_6); etc.

$$g_n = \frac{W_1-W_2}{S_n}, \qquad g_{n_1} = \frac{W_3-W_4}{S_n}, \quad \text{etc.}$$

If then g_n, g_{n1}, g_{n2}, etc., be deducted from (t_1-t_{11}), $(t_{11}-t_{12})$ and $(t_{12}-t_{13})$, etc., we get values $(t_1-t_{11})-g_n$, $(t_{11}-t_{12})-g_{n1}$, etc., which give successive temperature-falls per revolution. These are used to construct a second temperature-fall curve. This curve is shown in fig. 3 in dotted lines. By using the new values of the temperature-falls equal to the ordinates $c'n$, $e'n'_1$, $g'n'_2$, etc., another value of the specific heat is obtained for each expansion line, and a third temperature-fall curve is plotted. This curve, however, lies so close to the dotted curve that it is indistinguishable; and if the dotted curve be used the values so obtained of g_n, g_{n1}, g_{n2}, etc., are accurate within the error of experiment. By applying the same method to the points A, C, E, G and I, adding the temperature-fall equivalent of $(W-W_1)$; (W_2-W_3); etc., a temperature-fall curve is obtained calculated from the maximum volume, and such a curve is shown at fig. 4 in dotted lines. The specific heats on the different expansion lines are then obtained by (2)

$$S = \frac{W_1}{(t_1-t_{01})-g_1}, \quad \text{etc.}$$

It is found, however, that calculations made from the diagrams in this manner are liable to considerable disturbance due to possible indicator errors so small as 0·1 mm.

It was therefore considered desirable to make the specific heat calculations from measurements relating to the upper part of the diagrams, where the temperature differences to be measured are at a maximum.

In order to determine the law of heat-loss at the upper ends of the diagram, it is necessary to investigate the curve more closely.

If the true temperature-falls due to cooling on the double stroke be plotted with the mean temperatures of the working fluid during the double stroke, and the lines corresponding with the mean temperatures of the expansion stroke be drawn, cutting the curve so produced, it is found that the temperature-falls given by this curve at the mean temperatures of the expansion lines—divided by two to give the correct value for a single stroke—are the same as those found by the method above described from the dotted curve, fig. 3.

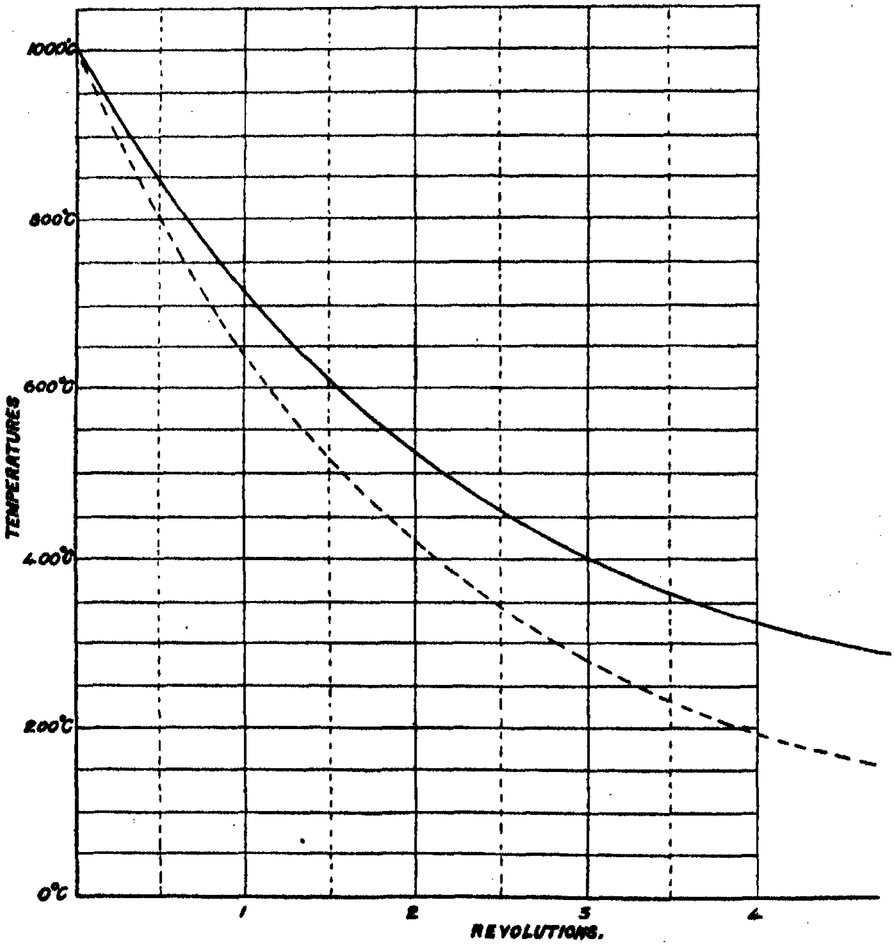


FIG. 4.

Fig. 5 shows such a curve; it has been obtained by measurement of mean temperatures on the successive pairs of compression and expansion lines, and by taking the values of temperature-fall for the double stroke from the dotted curve, fig. 3.

On fig. 5 the full vertical lines cutting the curve are drawn through points representing mean temperatures respectively on expansion and compression lines BC and CD, DE and EF, FG and GH, and HI and IJ. The vertical dotted lines refer to mean temperatures or expansion lines BC, DE, FG and HI only. The temperature-falls shown by the ordinates in full lines are taken from the dotted curve, fig. 3.

Numerous curves of this kind have been calculated, and prove that the

temperature-fall may without appreciable error be taken from the mean temperature curve in this way. This fact is applied to the determination of temperature-falls at the high-pressure part of the stroke as follows:—Referring to fig. 1, the vertical line marked 3/10 cuts the expansion and compression lines of the diagram at three-tenths of the engine stroke. This is the part which is to be investigated to determine specific heat. To distinguish the partial from the complete lines, the compression lines on fig. 1 are marked successively 1*c*, 2*c*, 3*c*, etc., and the expansion lines 1*e*, 2*e*, 3*e*, etc.

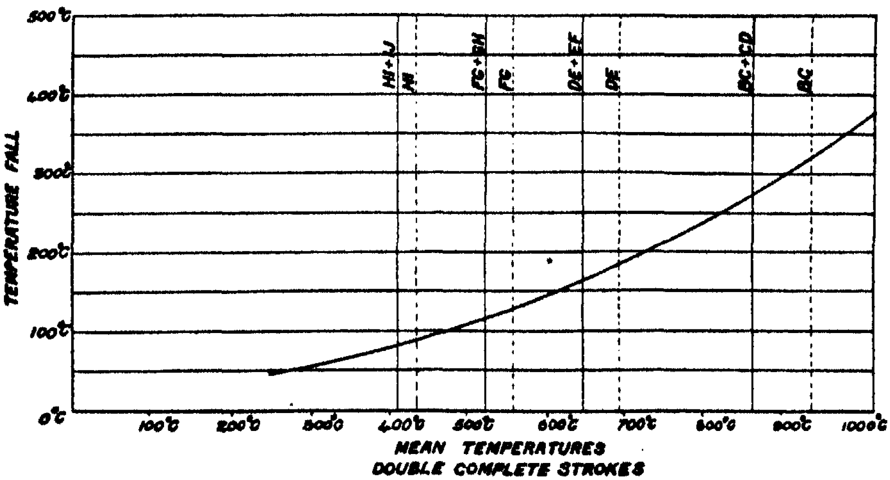


Fig. 5.

Work is done upon the gas equal to w on the first compression line 1*c*, and done by the gas on the piston by the first expansion line 1*e*, the work being w_1. The heat lost during the partial compression and expansion is thus measured by the temperature-fall between a and b plus the temperature-fall equivalent of the area $w - w_1$. By using the specific heat value already calculated for the complete expansion line, the value of the total temperature-fall g equivalent to the heat lost is obtained. The total temperature-fall has now to be divided between the compression and expansion lines. For this purpose the mean temperatures are calculated for 1*c* and 1*e*, and a mean temperature-fall curve drawn for the upper three-tenths stroke (see fig. 6).

On fig. 6 the full vertical lines cutting the curves are drawn through the mean temperatures respectively of the partial compression and expansion lines 1*c* 1*e*, 2*c* 2*e*, 3*c* 3*e*, and 4*c* 4*e*, and the dotted lines 1*e*, 2*e*, 3*e*, and 4*e*. A value, g_{1e}, for the temperature-fall on the expansion line 1*e* is taken from

the curve, fig. 6. The mean specific heat on the expansion line $1e$ is thus

$$S_{1e} = \frac{W_1}{(t_1 - t_b) - g_{1e}},$$

between the temperature limits t_1 and t_b. The other expansion lines are calculated in the same manner.

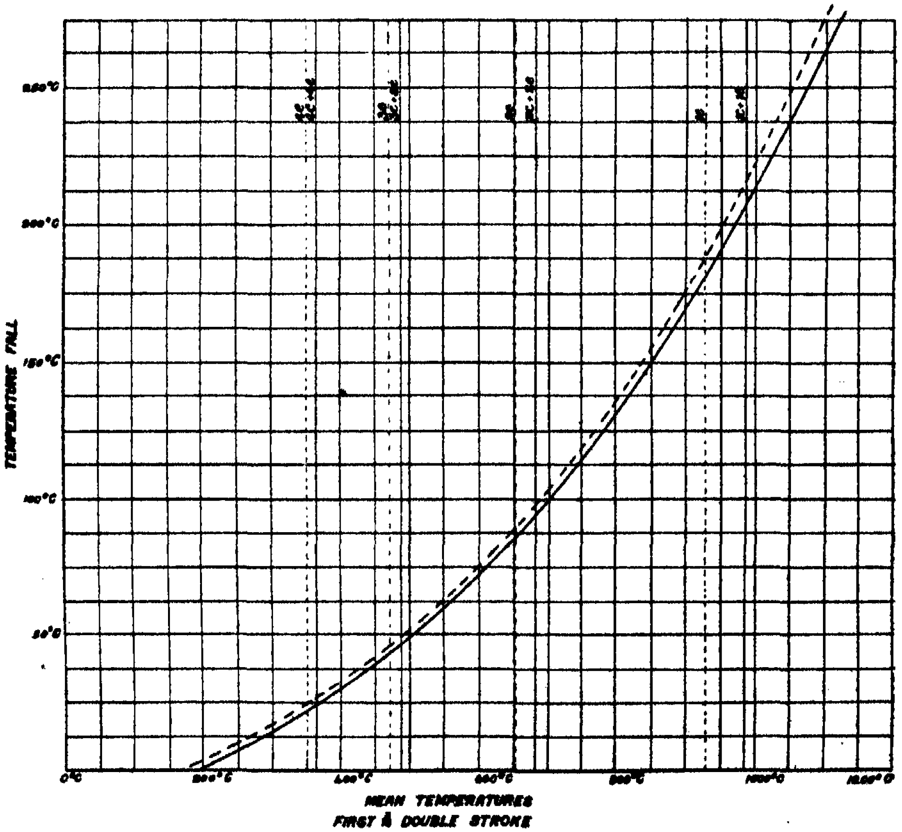


FIG. 6.

It was found that the specific-heat values obtained as above from the upper three-tenths diagram were lower than those obtained for the whole stroke. New temperature-fall curves were, therefore, drawn for each diagram, taking the average specific-heat value found as above described, on the upper three-tenths stroke. One of these curves is shown in dotted lines in the diagram 6.

The final specific-heat calculation was made, using these temperature-fall curves in a similar manner to that described above. It was found that the temperature-fall curves obtained by using the specific heat finally obtained

do not differ appreciably from the curves obtained when the average specific-heat value on the upper three-tenths stroke is used. It was, therefore, unnecessary to make a further calculation.

So much for the method of examination and calculation adopted. In what follows, the last described method of obtaining specific heat has been followed.

Engine and other Apparatus for Experiments.—The engine used for the experiments was of the well-known four-cycle type, 14 inches diameter cylinder and 22 inches stroke, designed for a full load of 60 brake horsepower at 160 revolutions per minute. It was constructed by the National Gas Engine Company, Limited, and was used by the Thermodynamic Standards Committee of the Institution of Civil Engineers, in conjunction with the present writer, for determination of data required for an ideal standard of comparison. It was carefully measured and calibrated by the Committee, the relevant matters being as follows:—

	cub. ins.
Volume swept by piston-stroke	3390
Total volume....................................	4164
Compression space	774
Compression per cent. of total volume ...	18·59

Fig. 7 shows a vertical longitudinal section and part horizontal section. The indicator used was of the Casertelli Richards type. It was carefully measured, and the springs calibrated, by the writer.

The coal-gas drawn into the cylinder was measured by a meter which had been tested by the Committee, and the air supplied was also measured by anemometer, also calibrated by the Committee. During the experiments, observations were made of temperatures of gas, air, and water flowing into and out of the engine water-jacket. Analyses of the coal-gas used were made, and the heat of its combustion was determined by the Junker calorimeter. No leakage could be detected past either piston or valves during the experiments.

Apparent Specific-Heat Values.—To determine the specific-heat values, the engine was run without load, and the governor adjusted to keep the speed at about 120 revolutions per minute. The total revolutions were determined by counter; the total number of explosions were also so determined. The water-jacket was kept cold by passing water through it at a sufficient rate. Gas consumption was taken, and barometer and thermometers read. The rate of engine revolutions was also taken by tachometer.

To make an observation the indicator cock was opened and a trigger operated,

after a gas charge had been taken in, to liberate the springs, pressing on the exhaust and inlet valve cam rollers. The springs displaced the rollers along their pins, placing them beyond the action of the cams. Consequently, the exhaust and inlet valves remained closed, so that although the electric spark produced the usual power-stroke explosion, the products of combustion produced were entirely retained in the cylinder and alternately compressed and expanded behind the piston, the engine being driven by the energy of rotation of the fly-wheel.

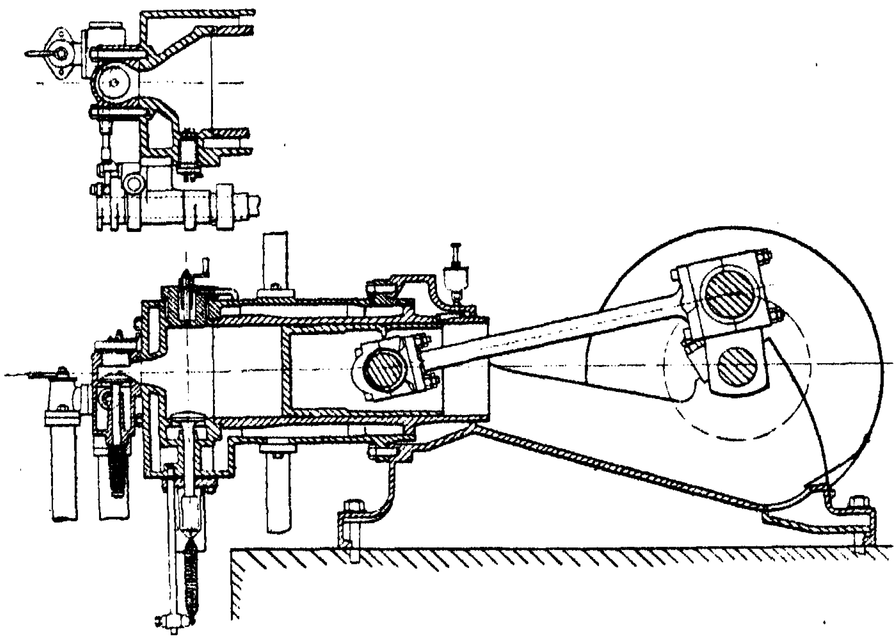


FIG. 7.

The indicator pencil was lightly held against the card during a number of revolutions and traced a series of gradually falling lines. The tachometer was observed and readings made at the moment of explosion, and at the moment of last contact with the card. In this way the actual time of each double operation was accurately known. During the first five revolutions after explosion the speed dropped from about 120 revolutions to 116 revolutions, so that the variation was only from 3 per cent. to 4 per cent. This variation, however, has been allowed for in preparing the cooling curves. Indicator diagrams were also taken with a light spring to determine the pressure of the charge within the cylinder on the completion of the charging stroke. It was found that the cylinder was entirely filled at this low

speed, so that the pressure within was the same as that of the external atmosphere.

Thirty indicator cards were taken at this speed for examination, and the proportions of the gas to air were varied within narrow limits to discover whether a slight change in composition changed the specific-heat values materially. The values of specific heat given are the mean values obtained from 21 cards, in which the mixture was of nearly the same proportions.

To facilitate measurement of the faint lines upon the cards, enlarged photographs were taken, doubling the scale. All measurements required for the calculations were made from these photographs. Enlargements Nos. 1 to 21 accompanying this paper (see footnote, p. 504), are those from which the apparent specific-heat values are deduced. In calculating from the diagrams, the following assumptions have been made:—

(1) That $PV/T =$ a constant for all temperatures and pressures of the experiments.

(2) That $PV =$ a constant for isothermal compression or expansion for all the experiments.

(3) That no chemical contraction or expansion has taken place due to combustion.

(4) That combustion is complete after seven-tenths of the first compression stroke.

Five expansion lines were examined on each card, and the various values described were calculated from carefully-made measurements.

The curve shown at fig. 8 was obtained from Cards 1 to 21 in this manner, so far as concerns the observed points *a*, *b*, *c*, *d*, *e*, each of which points is the average value of the numbers calculated from 21 cards. These points carry the apparent instantaneous specific-heat values up to 920° C. The point *f* was calculated in a similar manner from the upper one-tenth of the diagram, and the point *g* was obtained by similar processes from the explosion expansion line between one-tenth and three-tenths of the forward stroke. The observed points represent the average specific heats for the ranges of temperature of successive expansion lines, and the curve shown is so drawn that the average specific heats given by the curve over the same temperature ranges are equal to the observed values. The value at 0° C. is obtained by extrapolation, assuming all products to remain gaseous. The mean values from 0°—200° calculated from the usually accepted numbers are given, and it appears to be 20·25 where the curve shows 20·9. The specific heats are given in foot-pounds per cubic foot of working fluid

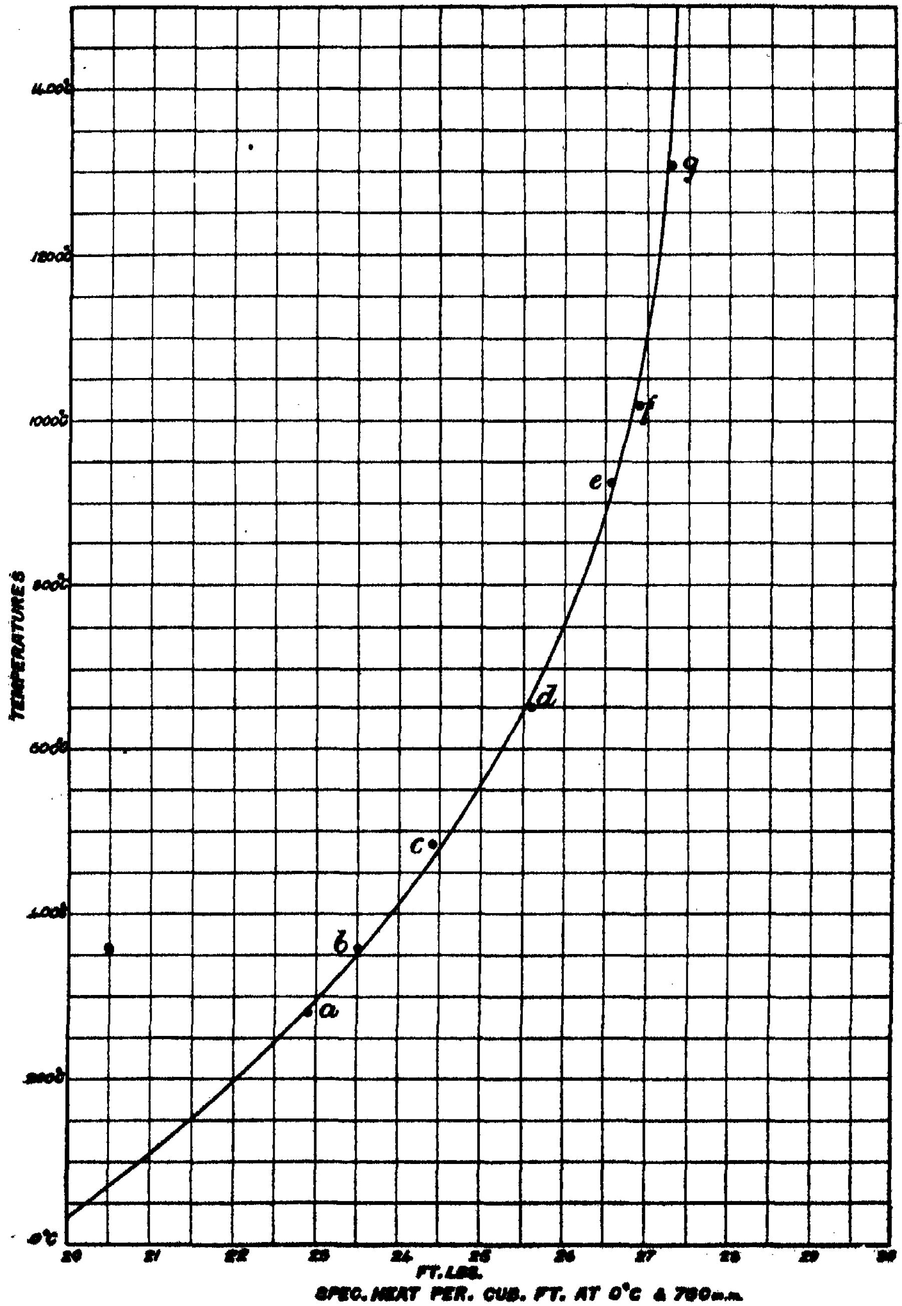

TEMPERATURES
0°C
200°
400°
600°
800°
1000°
1200°
1400°
20
21
22
23
24
25
26
27
28
29
30
FT. LBS.
SPEC. HEAT PER. CUB. FT. AT 0°C & 760m.m.
a
b
c
d
e
f
g

Fig. 8.

reduced to standard temperature and pressure 0° C., and 760 mm. mercury. The highest temperature measured for the purpose of this curve was 1450° C., and the lowest 250° C.

The mean composition by volume of the working fluid calculated from analysis of the coal-gas used at the works was

	Vols.
Steam (assumed gaseous) ...	11·9
Carbon dioxide	5·2
Oxygen	7·9
Nitrogen........................	75·0
	100·0

The mixture, however, was varied slightly between the extreme compositions, viz.:—

	Vols.		Vols.
Steam (assumed gaseous)......	11·2	and	12·7
Carbonic anhydride	4·8	"	5·5
Oxygen	8·7	"	7·0
Nitrogen	75·3	"	74·8
	100·0		100·0

These two extreme compositions correspond respectively to explosive mixtures containing before combustion 1 volume gas to 9·8 volumes air, and 1 volume gas to 8·5 volumes air.

The composition by volume of the coal-gas used was (average age of four analyses):—

Hydrogen	43·0
Marsh gas	33·2
Unsaturated hydrocarbons	4·3
Carbon monoxide	6·7
Nitrogen.............................	10·2
Oxygen	0·3
Carbon dioxide	2·3
	100·0

Table I shows the apparent instantaneous specific heats at different temperatures taken from the curve; and Table II shows mean apparent specific heats for temperature ranges from 0° C. up to 1500° C.

From these numbers it is evident that the apparent specific heat of the

working fluid consisting of products of combustion in the cylinder of the internal combustion engine increases considerably with temperature, so that at 1000° C. the value is 28 per cent. greater than it is at 100° C., while at 1500° C. the increase amounts to 31 per cent. The mean apparent specific heat between 0° and 1000° C. is 24·1 foot-pounds per cubic foot, or 15 per cent. greater than at 100° C., while at 1500° C. the increase amounts to over 20 per cent.

Table I from Fig. 8.—Table of Apparent Specific Heats (Instantaneous) in foot-pounds per cubic foot of Working Fluid at 0° C. and 760 mm.

Temperature.	Specific heat at constant volume.	Temperature.	Specific heat at constant volume.
° C.	ft.-lbs.	° C.	ft.-lbs.
0	19·6	800	26·2
100	20·9	900	26·6
200	22·0	1000	26·8
300	23·0	1100	27·0
400	23·9	1200	27·2
500	24·8	1300	27·3
600	25·2	1400	27·35
700	25·7	1500	27·45

Table II from Fig. 8.—Table of Mean Apparent Specific Heats in foot-pounds per cubic foot of Working Fluid at 0° C. and 760 mm.

Temperature.	Specific heat at constant volume.	Temperature.	Specific heat at constant volume.
° C.	ft.-lbs.	° C.	ft.-lbs.
0—100	20·3	0—900	23·9
0—200	20·9	0—1000	24·1
0—300	21·4	0—1100	24·4
0—400	21·9	0—1200	24·6
0—500	22·4	0—1300	24·8
0—600	22·8	0—1400	25·0
0—700	23·2	0—1500	25·2
0—800	23·6		

Inspection of the curve at once shows that while apparent specific heat increases more rapidly at first, it tends to a limit at the upper temperatures, so that from 1200° C. to 1500° C. the increase is less than half that from 900° C. to 1200° C. The appearance of the curve would suggest a limit for the apparent specific-heat value after no great further increase in temperature.

Is this a real increase of specific heat? If combustion be completed, then there appears to be no other explanation.

Consider, however, the points of difference which arise between a real change of specific heat with changing temperature and an apparent change caused by continued combustion. If the continued combustion be due to dissociation, then it would be impossible to differentiate by any experiments of this kind; but if it be combustion continuing at a given time rate, then discrimination is possible. With a real specific-heat change it is obvious that values calculated from any expansion line will show a fall along that line depending on temperature-fall only. No increase of specific heat could occur on the falling temperature expansion line. This is also true for change due to dissociation. If, however, combustion be continuing, then the apparent specific heat may vary from point to point of the line depending on the relations between the instantaneous values of the rates of heat addition to the working fluid and work performed by it, on the piston.

If the rate of heat addition be less than that of work performed, then the temperature will fall; but if the work rate diminishes more rapidly than the combustion rate, then at a certain point of the expansion the rates may become equal and then the expansion line may become isothermal, and it is even conceivable that temperature might rise. In such a case specific-heat values calculated from point to point of the supposed expansion line would show an increase with falling temperature, and at the isothermal point would become infinitely great. If combustion continues at a rate which becomes relatively greater than the work rate, it is evident then that specific-heat values will increase all along the expansion line as pressure and temperature falls. It has been already stated that this is found to be the case, but numerous calculations have been made from many diagrams which always show this interesting effect. To illustrate this point calculations have been made from Card No. 5, expansion line BC for: first three-tenths of stroke; first half of stroke; second half of stroke; and the whole stroke, as follows:—

Apparent Specific Heat.

First three-tenths of stroke...	26·3 ft.-lbs.	per cubic foot	at 0° C.	and 760 mm.
First half of stroke............	27·3	"	"	"
Second half of stroke.........	34·3	"	"	"
Whole stroke	28·4	"	"	"

Other cards show greater differences between the first three-tenths and the later parts of the stroke, some as much as 50 per cent. increase. It is true

that difficulties of reading and errors of indicator affect the lower ends of the diagram more seriously than the upper, and some allowance must be made for this; but notwithstanding this, the difference between the early parts of the stroke are so marked that it appears very improbable that they can be accounted for by any such errors. Combustion appears to be proceeding.

Other indications are given by varying the rate of revolution of the engine. If specific heat varies only with temperature, then obviously change in engine speed cannot alter specific-heat values. To test this matter four cards were taken at 160 revolutions, and four at 120 revolutions per minute, and calculations were made on expansion line BC for whole stroke, first half stroke and first three-tenths stroke. Results were obtained as follows:—

Apparent Specific Heat.

	160 revs.	120 revs.
Whole stroke	34·4 ft.-lbs.	31·0 ft.-lbs.
First half stroke	29·5 „	28·3 „
First three-tenths stroke ...	27·2 „	26·6 „

This experiment clearly indicates an increase of apparent specific heat with increasing speed as well as increased apparent value with falling pressure. The effect of increasing speed to some extent introduces opposing changes; higher speed means less combustion completed when expansion on the line BC begins, but it also means performance of work on the piston at a greater rate, so that the new factors cancel each other out to some extent.

Still another indication is given by varying the temperature of the water-jacket of the engine, keeping the speed of the engine constant. The experiments were made with the engine running at 120 revs. per minute with the water-jacket cold, 13° C., and hot, 66° C.

Three expansion lines were calculated for the first three-tenths stroke. For cold, Cards 1 to 21 were taken; the values given are mean values of the 21 cards. For the hot experiments eight cards were taken; the values given are means of eight. Results were obtained as follows:—

Apparent Specific Heat.

	Water-jacket, 13° C.	Water-jacket, 66° C.
1*e*	26·6 ft.-lbs.	23·9 ft.-lbs.
2*e*	25·2 „	22·6 „
3*e*	23·9 „	21·9 „

Obviously apparent specific heat is less with the hot water-jacket. This could not affect the matter if the only change with temperature is in specific

heat. The cause of the change may be due to the rise of the mean temperature of the chemical reaction which permits chemical action to be less affected by cooling walls which tend to remove a portion of the contents from the sphere of effective reaction by undue cooling. That is, combustion proceeds more quickly in a hot vessel than a cold one. This would produce the effect of lowered specific-heat value by leaving less heat to be evolved when expansion line BC commenced.

The experiments appear, therefore, to prove that some combustion is proceeding. The nature of the apparent specific heat also changes, as the rate of apparent change is much greater at the lower temperatures, taking the same portion of the stroke.

Experiments made by the Thermodynamic Standards Committee of the Institution of Civil Engineers in conjunction with the present writer appear to support the contrary conclusion, because in this same engine a balance-sheet was determined as follows for 100 heat-units given to the engine.

Indicated work	35·00
*Heat-loss to water-jacket and radiation..........................	28·00
*Heat passing away with exhaust determined by calorimeter	38·5
	101·5

This appears to account for the whole of the heat at the conclusion of the return or exhaust stroke. The writer considers, however, that the method used to determine radiation and the exhaust calorimeter might introduce an error of 5 per cent. and possibly more. It is obvious that further experiment is required to settle the question.

Heat-Flow Values.—Curves for temperature-fall values were prepared from the 21 cards used for the determination of apparent specific heat, each curve arranged as described to show temperature-fall in terms of mean temperature of the various expansion and compression lines studied. For convenience of calculation the temperature-fall values were expressed in isothermal units. The method of isothermal units consisted in referring all temperatures to an isothermal line calculated for the particular proportions of the cylinder and compression space. The corrections for temperature-fall were applied to the expansion lines of the same card. As the correct estimation of temperature-fall is important, and the accuracy of the specific-heat values depends upon

* It is known that part of the heat which should appear in the exhaust calorimeter passes into the water-jacket, so that the jacket loss value is too high, while the exhaust loss is too low.

its knowledge, it has been thought desirable to study more closely the mode of heat-loss in the cylinder, and some features which call for explanation.

Fig. 9 has been prepared with this object. Four curves are shown. The curves *a*, *b*, represent the heat-losses incurred in complete revolutions, that is, in complete double strokes. Here the surface exposed and covered alternately is that due to the whole sweep of the piston. The curves *a′*, *b′*, represent losses incurred at the upper three-tenths of the card (see fig. 1) while the piston moves from three-tenths stroke to the end, compressing into the clearance space, and then moves out again to the point of three-tenths of the outward stroke. The ordinates give heat-loss in foot-pounds per second, and the abscissæ mean temperatures per double stroke or double three-tenths stroke.

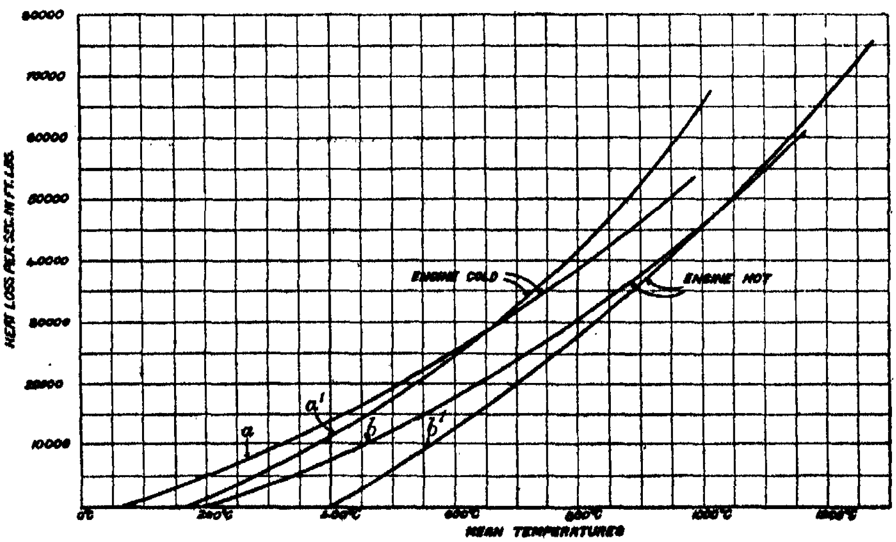


Fig. 9.

Curves *a* and *a′* are calculated from Cards 2, 3 and 5, taken while the engine was running without load at 120 revolutions per minute with the jacket water kept at a mean temperature of 13° C. Curves *b* and *b′* are calculated from Cards Nos. 22, 23 and 24, taken while the engine was running at about 160 revolutions per minute with a load of 50 B.H.P., and the jacket water at 80° C. Accordingly the curves are marked as engine cold and engine hot.

The heat-loss values given are calculated from temperature-fall curves, assuming the apparent specific heats given at fig. 8 and at Tables I and II to be real specific heats; but for the amounts of continued combustion believed

to be present it appears probable that the heat-flow values calculated are also sufficiently close approximations within the errors of experiment.

Taking curve *a*, it appears that the heat-loss is not proportional to temperature difference, but increases more rapidly as temperature rises. Further, the curve, when prolonged to the zero line representing no loss of heat, cuts that line at the temperature 65° C. This appears to show that during complete engine strokes, cooling ceases at that temperature, so that the mean temperature of the inner surface of the enclosing walls must be about 65° C., notwithstanding the fact that the water in the jacket is at 13° C.

Curve *a′* shows that the rate of heat-loss in time-units is greater for the first three-tenths of the engine stroke, even with equal mean temperatures. The curve cuts the zero line at a temperature of 165° C., showing that the mean temperature of the wall surface is higher for the inner part than for the whole stroke. This is to be expected for two reasons: the piston end, valves and caps are not water-jacketed, and their surface temperature is doubtless higher than the jacketed part, so that the inner portion presents a larger proportion of hot surface; also the mean temperatures of gases at the inner end are higher than those at the outer end, so that the surfaces must be hotter than those exposed at the outward end of the stroke. This fact explains the smaller heat-loss at the lower temperatures. The crossing of the line *a* by *a′* proves the greater rate of heat-loss at the inner end; this greater heat-loss appears to be due partly to the greater mean density of the gaseous contents at the inner end, and partly to the larger proportion of the cooling surface of the admission port to the whole surface towards the inner end.

Taking curve *b* for whole stroke and hotter cylinder and comparing it with curve *b′* similar relations are apparent, but here the mean temperatures of the wall-surface are much higher—190° C. for curve *b* (whole stroke), and 400° C. for curve *b′* (three-tenths stroke). The total surface exposed when the piston is full out is approximately 11·2 square feet, and at three-tenths out about 6·5 square feet. Calculating for equal temperature differences, it is found that the heat-flow per square foot per second at the three-tenths end is from 2·5 to 2·8 times that obtained for the whole stroke.

From this follows the necessity of determining the heat-flow separately for each part of the expansion curve investigated; the quantitative law of heat-flow varies with surface exposed and density of working fluid, as well as with temperature. Every part of the stroke will give distinct heat-flow values. For three-tenths stroke, for example, a temperature difference of 700° C. causes a heat-flow of 8537 foot-lbs. per square foot per second, while

for the whole stroke 700° C. difference only gives 3340 foot-lbs. per square foot per second. That is, increase of density and change of configuration are together responsible for the greatly increased rate of heat-loss per unit surface in unit time.

Calculation of the Total Heat given to the Combustible Mixture from the Indicator Diagram only.—If the results given as to apparent specific-heat, temperature-fall, and heat-flow values be correct, then the total heat given to the combustible mixture may be calculated by diagram only. To test this, three diagrams were taken from the engine while it was running at 160 revolutions per minute under a load of 50 brake horse-power. The diagrams are numbered 22 to 24. The heat-flow determinations were made as described, and plotted in work units per whole stroke.

Fig. 10 shows the mean curve. The apparent specific-heat values were calculated for the proper ranges of temperature from the curve, fig. 8. The charge temperature before compression was determined by a method which need not be described here ; it was 95° C.

Three balance-sheets were calculated from the data as follows :—

Card No. 22.

	ft.-lbs.	Per cent.
Heat-flow during explosion and expansion	12,480	15·4
Heat contained in gases at end of expansion	39,800	49·0
Indicated work	28,900	35·6
Total heat	81,180	

i.e., 104 B.T.U.

Card No. 23.

	ft.-lbs.	Per cent.
Heat-flow during explosion and expansion	14,000	17·0
Heat contained in gases at end of expansion	40,500	49·3
Indicated work	27,700	33·7
Total heat	82,200	

i.e., 106 B.T.U.

Card No. 24.

	ft.-lbs.	Per cent.
Heat-flow during explosion and expansion	13,100	16·0
Heat contained in gases at end of expansion	40,600	49·5
Indicated work	28,260	34·5
Total heat	81,960	

i.e., 106 B.T.U.

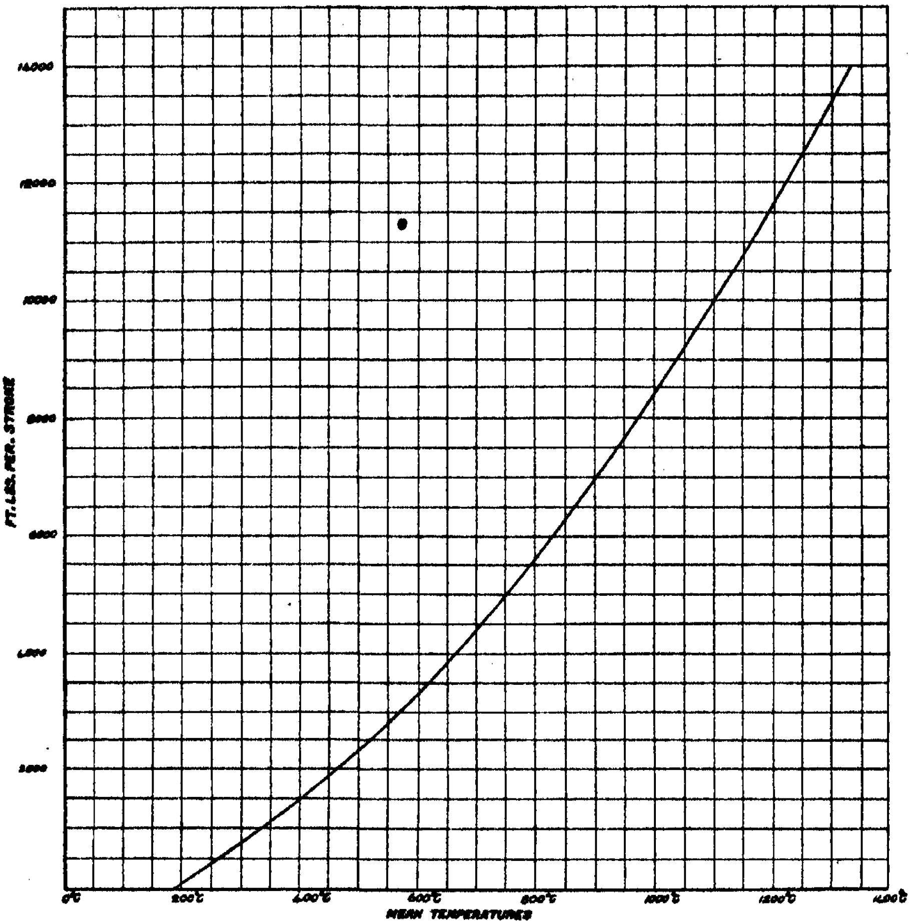


FIG. 10.

These values give the total heat accounted for by the indicator diagram of this particular internal combustion engine, and its distribution in indicated work, necessary exhaust-loss and heat-flow through the cylinder walls.

The diagram informs us that in all 104 to 106 British thermal units have been given to the gases in the cylinder for each power explosion. If this method be correct, then the total heat so found should correspond with that known to be present from the measurement of gas supply. The coal-gas present in the mixture per explosion was 0·183 cubic foot. Its lower heat value was 574 B.T.U per cubic foot.

$$0{\cdot}183 \times 574 = 105 \text{ B.T.U.}$$

The diagram thus accounts for the 105 B.T.U. known to be present in the form of coal-gas.

Calculations from other indicator diagrams confirm the correspondence of the diagram calculated heat values with those determined from the heat values of the gas known to be present.

These apparent specific-heat and heat-flow values now make it possible for the first time to study the thermodynamic problems of the internal combustion motor from the indicator diagram only, and this the writer believes will materially hasten the development of a complete theory of these motors by making it possible to determine the principal properties of flame in the engine cylinder itself. Many obscure phenomena are capable of investigation by the method.

Earlier Investigations.—Many chemists and physicists have experimented with gaseous explosions, including Hirn, Bunsen, Mallard and Le Chatelier, Berthelot and Vieille, Witz, Dixon, Clerk, Grover, Petavel, and Bairstow and Alexander. All have observed a deficit of pressure for heat of combustion known to be available; many explanations have been offered, as—cooling loss to walls, dissociation of steam and carbon dioxide, increase in specific heat, and combustion at a time rate.

In 1886 the present writer made experiments with coal-gas and hydrogen explosions with air, and he stated that "the author's experiments prove that the generalisations arrived at by chemists, for the very slow chemical actions investigated by them at low temperatures, hold equally good in the case of rapid chemical combinations occurring at high temperatures producing explosion. In a rich mixture, where the acting gases are but little diluted by neutral gas, the combination is at first exceedingly rapid, but becomes slower as it proceeds, because of the diluting effect of the products. In a poor mixture, when the molecules of the acting gases are widely separated by diluent, the combustion is slow from the first."

This position is now very generally accepted among investigators in chemical physics, and it is undoubtedly true of rapid as well as slow combinations. When the writer began the present investigation, he believed that these phenomena of slower chemical action furnished a complete explanation of the discord between the theoretical and observed results, and that there was no need to assume any considerable dissociation or variation of specific heat of the products of combustion. These experiments, however, appear to him to indicate real change of specific heat as well as continuation of combustion. The experiments do not exclude dissociation or any other molecular change which by requiring the performance of work would change specific heat. It appears improbable, however, that dissociation should be

material for temperatures so low as 600° C. It is not usual to suppose that either carbon dioxide or steam can be decomposed to any sensible extent at such temperatures.

Mallard and Le Chatelier developed the theory of specific-heat change fully in 1883 from their experiments on explosion in closed vessels, and they give the following formulæ for the mean specific molecular heat at constant volume for carbon dioxide, nitrogen, and oxygen:—

$$\begin{array}{ll} \text{For } CO_2 \ldots\ldots\ldots\ldots & C_v = 6{\cdot}26 + 0{\cdot}367t \times 10^{-2}. \\ H_2O \ldots\ldots\ldots\ldots & C_v = 5{\cdot}61 + 3{\cdot}28t \times 10^{-3}. \\ N \text{ and } O \ldots \ldots & C_v = 4{\cdot}8 + 0{\cdot}006t. \end{array}$$

Dividing respectively by 44, 18, 28, and 32, the molecular weights of carbon dioxide, steam, nitrogen, and oxygen, we get—

$$\begin{array}{ll} \text{For } CO_2 \ldots\ldots\ldots\ldots & C_v = 0{\cdot}1423 + 0{\cdot}0000834t. \\ H_2O \ldots\ldots\ldots\ldots & C_v = 0{\cdot}3116 + 0{\cdot}000182t. \\ N \ldots\ldots\ldots\ldots\ldots & C_v = 0{\cdot}171 + 0{\cdot}0000215t. \\ O \ldots\ldots\ldots\ldots\ldots & C_v = 0{\cdot}150 + 0{\cdot}0000188t. \end{array}$$

These formulæ obviously show that the change of specific heat of the working fluid should be represented by a straight line, and this does not agree with the present experiments. These values, however, agree fairly with the apparent specific heat at 1000° C. and 1500° C.

According to Mallard and Le Chatelier's figures, the mean specific heat of the working fluid used in the present experiments at 1000° C. would be 23·5 foot-lbs. per cubic foot at 0° C. and 760 mm. mercury. The value from Table II of this paper is 24·1 foot-lbs. For 1500° C., Mallard and Le Chatelier works out 25·5 foot-lbs., Table II 25·2 foot-lbs. Mallard and Le Chatelier's observations did not go below 1000° C., as explosions became too slow near that temperature, so that they have estimated specific heats at the lower temperatures by extending the use of the formulæ considerably below the points of observation.

Holborn and Austin have determined the specific heat of carbon dioxide, oxygen, nitrogen and air, at constant pressure, by means of a heating appliance, a thermo-couple, and a calorimeter; but their values are considerably lower than those of Mallard and Le Chatelier, and show a much smaller proportion of increase for CO_2, N, and O, but their numbers appear to be quite consistent with these experiments.

Conclusions.—(1) The apparent specific heat of the working fluid of the internal combustion engine (consisting mainly of a mixture of nitrogen, carbon dioxide, steam and oxygen), when calculated from the first three-

tenths of the engine stroke, undoubtedly increases between the observed temperatures 300° C. and 1500° C., but tends to a limit at the upper temperature.

(2) The apparent change in specific heat is not entirely due to a real change of specific heat, but requires in addition continuing combustion to account for all the facts.

(3) The rate of heat-flow from the working fluid to its enclosing walls for equal temperature differences varies throughout the stroke. Increased heat-flow accompanies increased mean density.

(4) The mean temperature of the inner surface of the enclosing walls varies with the portion of the stroke examined from 190° C. for whole stroke to 400° C. for first three-tenths stroke under working conditions at full load. These mean temperatures, however, are not the highest mean temperatures reached by the walls.

(5) The heat distribution during the operations of the working fluid can be determined with approximate accuracy from the apparent specific-heat values and heat-flow values obtained from the diagram only.

The Richards Casertelli indicator used by the writer for these experiments is one of the best adapted to stand the rough usage of frequent heavy explosions rising above 400 lbs. per square inch; but its indications at the lower pressures are not sufficiently accurate for the purpose of discussing the law of the variation of apparent specific heat throughout the stroke. For this purpose it is desirable to design an indicator of a different type. The writer has examined the leading existing indicators, including the optical instruments of Hospitalier, Carpentier and Petavel, but finds all unsuitable for the delicate work required in order to attain further accuracy in this investigation. The writer has accordingly designed a novel type of mechanical and optical indicator, with which he hopes to obtain diagrams which are sufficiently accurate at the low as well as the high pressures. When these further experiments are made, he trusts that he will be able to distinguish with quantitative accuracy between the phenomena of changing specific heat and continuing combustion.

The writer would point out that the method of investigating specific heat, temperature-fall, and heat-loss here developed is applicable to determinations of the specific heat of gases, heated without combustion. These experiments on the compression of practically a mass of flame show that even at high temperatures the heat-flow to a cylinder is lower on the whole than the rate of addition of heat to the mass of gas by performing work upon it. For example, in many of these experiments one complete compression occupying 0·25 second raises the temperature of the contents of the cylinder from

1000° C. to 1300° C., showing that the rate of heat-flow from the gas, even at a mean temperature of over 1000° C., is considerably less than the rate at which heat can be added to the gas by work performed. From this indication it is evident that a simple gas, such as nitrogen, oxygen, or a compound such as carbonic acid, could be heated by compression alone, in a suitable apparatus, to at least 1500° C. It only requires a sufficiently powerful mechanical apparatus to stand the high pressure of about $1\frac{1}{2}$ tons per square inch to get any desired temperatures. Such determinations will be entirely free from doubt due to possible combustion. The method described requires some modification, owing to the fact that where all the heat is added by compression, at one part of the stroke the gas would be absorbing heat, and at another part giving it out.

My thanks are due to my assistant, Mr. W. Grylls Adams, M.A., for his very effective aid in the laborious work of measuring the diagrams and making the numerical calculations. I have also had much pleasure in discussing with him the somewhat numerous points of difficulty which have arisen in the course of the investigations.

APPENDIX.—Received March 24, 1906.

Mean Temperature.—In the foregoing mean temperature during any expansion or compression stroke or part of a stroke is taken in relation to time, the obliquity of the connecting rod being neglected and the motion of the piston being taken as simple harmonic motion. The error introduced by this asssumption is negligible within the accuracy of the experiments.

Division of Heat-Loss between Expansion and Compression Lines.—It will be noticed that the temperature-fall due to heat-loss is divided between compression and expansion lines by drawing a curve whose abscissæ represent mean temperatures, and whose ordinates represent the temperature-fall, and taking the temperature-fall on any expansion line from this curve at the mean temperature of that expansion line.

This involves the assumption that heat-loss would be the same on an expansion line, and on a compression line for the same part of the stroke if the mean temperatures of the two lines were the same.

In considering the whole strokes, the temperature-fall due to heat-loss on an expansion line can be obtained by considering the expansion line with the compression line above it, and also with the compression line below it. This has been done, and it is found that the value thus obtained for the line BC (fig. 2) is the same within the error of experiment, whether it be arrived at by considering the pair of lines AB, BC, or the pair of lines BC, CD, and similar results are obtained for the other lines. This shows that the

temperature-fall due to heat-loss on pairs of lines starting from the lower end of the diagram, can be taken from the temperature-fall curve obtained by considering the pairs of lines starting from the upper end, and hence the value for an expansion line or compression line can also be taken from the curve.

In the consideration of the upper end of the stroke, the case is somewhat more difficult, as the time periods dealt with are not continuous; there will be a difference between compression and expansion strokes, in that on a compression stroke at the upper end of the diagram the gases in contact with the cylinder walls have during the immediately preceding part of the stroke been at a lower temperature, while on the expansion stroke they have been at a higher temperature. On the upper three-tenths stroke therefore the cylinder walls will be at a slightly lower temperature at the beginning of the part of the compression stroke considered than their temperature at the beginning of the part of the expansion stroke considered. The difference of temperature will not be great, and the error introduced by neglecting the difference is a very small one. This error also produces very little effect on the apparent specific-heat value; an error of 10 per cent. in dividing the temperature-fall due to heat-loss between the compression and expansion lines would not produce more than 3 per cent. error in the specific-heat value.

Errors of Reading, Indicator, etc.—In the reduction to normal pressure and temperature no account has been taken of the change of volume due to combustion. This change is small and only affects absolute values, and does not appreciably interfere with the deductions based on comparison of the various figures obtained.

In calculating from the upper three-tenths stroke, an error of 1/200-inch on the indicator card, and of 1/100-inch on the enlarged photograph, would introduce an error of 3 per cent. on the specific-heat value obtained on the first expansion line, and an increasing error on successive expansion lines amounting to 5 per cent. on the fourth expansion line.

In calculating from the whole stroke an error of 1/200-inch on the indicator card, and 1/100-inch on the enlarged photograph would introduce an error of 4 per cent. in the specific heat obtained from the first expansion line.

In order to obtain the comparative results given from different parts of the stroke by errors of indicator, etc., it would be necessary to make an error amounting to about 1/50-inch on the indicator cards and 1/25-inch on the photographs.

There is nothing on the diagrams to suggest such an error as this, and the results are not of the kind that would be produced by indicator slack

or friction. This can be shown by calculating the specific heat on the same expansion line, taking the heat-loss correction from the next upper and next lower compression lines respectively. It is found that the results are practically the same in the two cases.

The effect of friction and slack is to keep the expansion lines uniformly higher than they should be, while the compression lines are kept lower than they should be.

The result of this is that a temperature-fall measured on an expansion line will be too small, as the same height measured vertically will be equivalent to a greater temperature difference at the lower parts of the diagram; and similarly a temperature-rise measured on a compression line will be too great. The result as regards the specific-heat value found from the upper three-tenths stroke is that frictional error on the expansion line is to a certain extent if not wholly balanced by frictional error on the compression line. This arises in the following manner:—

The error on the expansion line causes the gross temperature-fall on expansion to be too small, but to get the temperature-fall which is equivalent to the work under the expansion line we subtract from the gross temperature-fall a temperature-fall due to cooling, the determining part of which is the temperature difference measured between the compression and expansion lines on the ordinate at the three-tenths stroke. This temperature difference is thus taken too small, and approximately too small by the same amount as the gross temperature-fall is too small.

In calculating the specific heat on a given expansion line a different effect is produced by friction, according to the compression line from which the correction is made for cooling. If an expansion line is considered in connection with the compression line immediately below it, friction causes these two lines to be too far apart instead of too near together. The temperature-fall measured on the expansion line is, as before, too small, but the temperature-fall due to cooling is in this case too great, and therefore the friction will produce an accumulated effect.

The fact that the value obtained for specific heat for the whole expansion line is the same whether that expansion line is considered together with the compression line above it, or with the compression line below it, therefore seems to show that friction and indicator-slack could not be the cause of the results obtained.

The Stability of Submarines.

By Sir William H. White, K.C.B., F.R.S.

(Received April 4,—Read May 3, 1906.)

The purpose of this paper is to place on record the results of calculations made to determine the conditions of stability of submarine vessels in varying circumstances which may occur on service. Accidents have happened to many submarines, and in some instances have been accompanied by loss of life. After investigating possible causes of accident, the author was convinced that one of the chief was the singular variation in stability and buoyancy produced by changes in the draught of water and the "trim" of submarines. He was led, therefore, to undertake the detailed calculations of which the principal results are now stated and illustrated.

Either by accident or intention, submarines may reach considerable depths below the surface and be exposed to severe external fluid pressures. Ample structural strength must be provided to meet these pressures and to prevent deformation of the vessels. In order to fulfil this object with moderate weights of structures, submarines are made "cigar-shaped," with circular or nearly circular cross-sections. The cigar-shape is usually somewhat disguised by light superstructures built above the upper surface of the hull proper, and carrying decks or platforms, which add to the comfort and convenience of the crews when the vessels are floating at the surface—in the "awash" condition—at their lightest draught. In that condition water is excluded from the spaces between the superstructures and the cigar-shaped hulls, and the buoyancy and stability are sensibly increased.

The other extreme condition at the surface is that when a submarine has been "trimmed" for diving, and floats with a very small portion of her hull above water. This is effected by admitting water-ballast into tanks specially constructed for the purpose and of known capacity. The final adjustments of draught and trim during the process of trimming require great care. All openings into the interior are closed and secured in a watertight manner before trimming is commenced. Water is also allowed to enter the spaces between the superstructures and the cigar-shaped hull, and to remain in free communication with the surrounding water, so that the lightly constructed superstructures may sustain no external pressure when the vessel is submerged.

Diving is accomplished by giving the submarine headway, and so manipulating horizontal rudders that the bow is depressed. The "stream-lines"

developed in the water by the onward motion produce downward pressures on the upper surface of the hull towards the bow; the vertical component of these pressures overcomes the vertical component of the rudder pressures and the small "reserve of buoyancy" which the submarine retains, and the vessel moves obliquely downwards until the desired depth below the surface is reached. The horizontal rudders must be then manipulated by a skilled steersman in such a manner that further motion (although really along an undulating course) is practically at a constant depth below the surface. When headway ceases, both rudder pressure and stream-line motions disappear, the small reserve of buoyancy reasserts itself and the submarine rises to the surface.

This general statement may be illustrated by figures for an actual submarine, resting on official evidence given at the enquiry into the foundering of submarine A8 at Plymouth last year.

In the awash condition, at the lightest draught of water, the reserve of buoyancy was about 13 tons (excluding the conning tower), the corresponding displacement exceeding 200 tons; so that the maximum reserve of buoyancy was about 6 per cent. of the displacement. The minimum reserve of buoyancy accepted for any class of war-ships at their deep-load draught has been about 10 per cent. in low-freeboard American "monitors," many of which vessels foundered. For "breastwork monitors" in the Royal Navy the corresponding reserve was 30 per cent. of the load displacement; for high-freeboard war-ships and passenger steamers it is from 80 to 100 per cent.; for cargo steamers it varies from 25 to 40 per cent. The contrast between submarines at their lightest draught and other types of ships at their deepest draught, shown by these figures, indicates the acceptance of altogether exceptional conditions in submarines, and the necessity for their cautious management in the awash condition at the surface, when the apertures on the upper surfaces are kept open.

These apertures are closed and secured before the vessels are trimmed for diving by admitting water-ballast. In the diving condition the reserve of buoyancy is extremely small. For submarine A8 it is said to have been 800 pounds, the corresponding displacement being about 220 tons. In other submarines of about the same displacement the reserve of buoyancy in the diving condition has been only 300 to 400 pounds. Consequently there is a necessity for extreme care in the final stages of trimming.

The cigar-shape of the hulls involves very rapid changes in the areas and moments of inertia of the planes of flotation as the draught of water is increased in passing from the awash to the diving condition: the stability is greatly reduced, and every member of the crew has to remain in his station.

No weights must be allowed to shift. All the conditions, in fact, differ from those which prevail in ships of ordinary form as they pass from the extreme light draught to the deep-load, for in such ships the outlines of transverse sections approximate to the vertical, except near the bow and stern, over the range between these extreme conditions, and the areas and moments of inertia of planes of flotation do not vary greatly.

These general statements may be illustrated by the comparison of a small cruiser of ordinary form with a submarine. The cruiser is about 260 feet long at the water-line, 37 feet broad, and 14 feet 6 inches mean load-draught, the corresponding displacement being about 2000 tons. The submarine has an extreme length of 150 feet, is 12·2 feet in extreme breadth, and has a displacement of 300 tons in the diving condition. In the light (awash) condition the submarine draws about 18 inches less water than in the diving condition, and has a displacement of about 284 tons. When awash the length at the water-line is 94 feet, and breadth extreme 8·2 feet; when in the diving condition the corresponding measurements are 41 feet length and 3·6 feet breadth. These figures differ widely from the length of 150 feet over all and 12·2 feet maximum breadth. In the cruiser, within the corresponding range of draught (18 inches) there is practically no change in length and breadth extreme at water-line, and these dimensions are practically identical with the extreme dimensions of the vessels. Fig. 1 illustrates the contrast

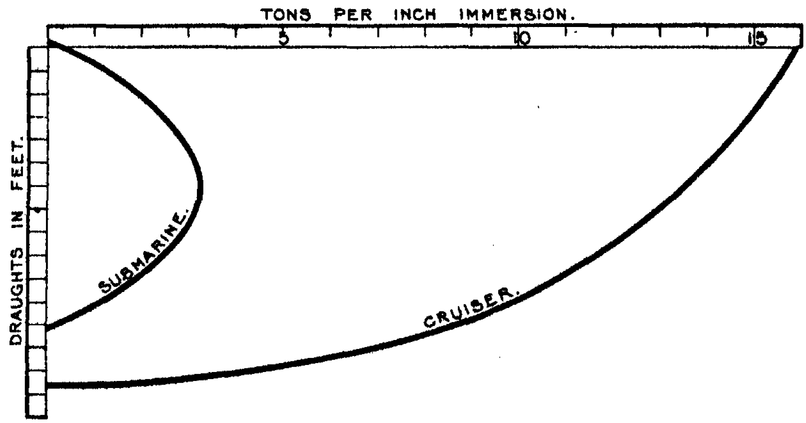


Fig. 1.

between the cigar-shape and the ordinary ship-shape. Horizontal measurements to the curves on that diagram, at any draught of water, measure the area of the corresponding plane of flotation, and the number of tons required to immerse the vessel one inch. It is obvious that the small area of the plane

of flotation at the lightest draught, its rapid diminution as the draught is increased, and the critical condition when trimmed for diving, all render possible the establishment of vertical dipping oscillations in submarines by comparatively trifling disturbances in the water-surface surrounding them.

Fig. 2 shows the "metacentric diagrams" for *transverse* inclinations of the two vessels, constructed in the usual manner. MM shows the *locus* of the metacentre of the cruiser, and BB that of the centre of buoyancy as the draught of water varies. The curves *m*, *m*, *m* and *b*, *b*, *b* show the corresponding *loci* for the submarine. The intercept between these curves on any vertical ordinate represents the height of the metacentre above the centre of

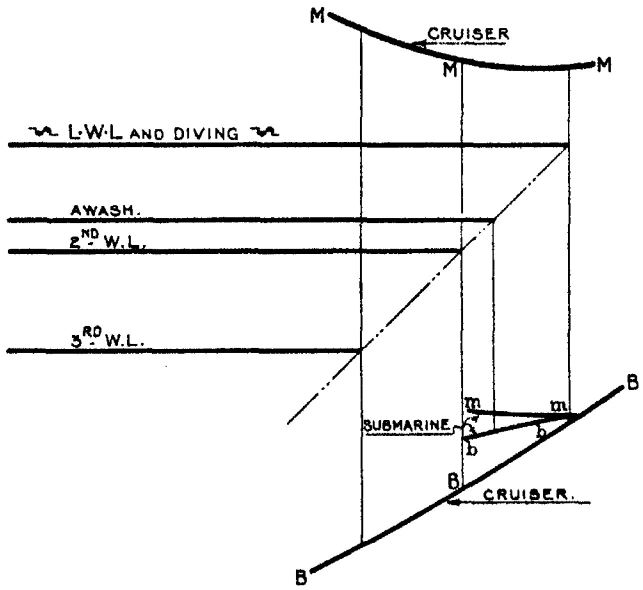


FIG. 2.

buoyancy at the corresponding draught of water. For the submarine in the awash condition this height is 0·32 foot, and in the diving condition it is 0·01 foot. Stability is obtained by disposing the weights so that the centre of gravity of the vessel and its contents lie below the axis; and in some existing submarines in their diving condition the vertical distance between the axis and the centre of gravity, or metacentric height, is said to be about 9 inches. When submerged, this measure of stability, of course, applies to inclinations from the vertical in any direction.

For the cruiser the height of the metacentre above the centre of buoyancy is 7·7 feet at the load water-line, and 8·6 feet for the water-line (18 inches

below the load) corresponding to the awash condition of the submarine. The centre of gravity of the cruiser is about 2 feet below the metacentre and 5¾ feet above the centre of buoyancy in the load condition; the metacentric *locus* is nearly horizontal from the load to the light condition, and the centre of gravity rises a few inches as coals and stores are consumed.

Fig. 3 shows the metracentric diagrams for *longitudinal* inclinations. For the submarine awash the metacentre is 37 feet above the centre of buoyancy;

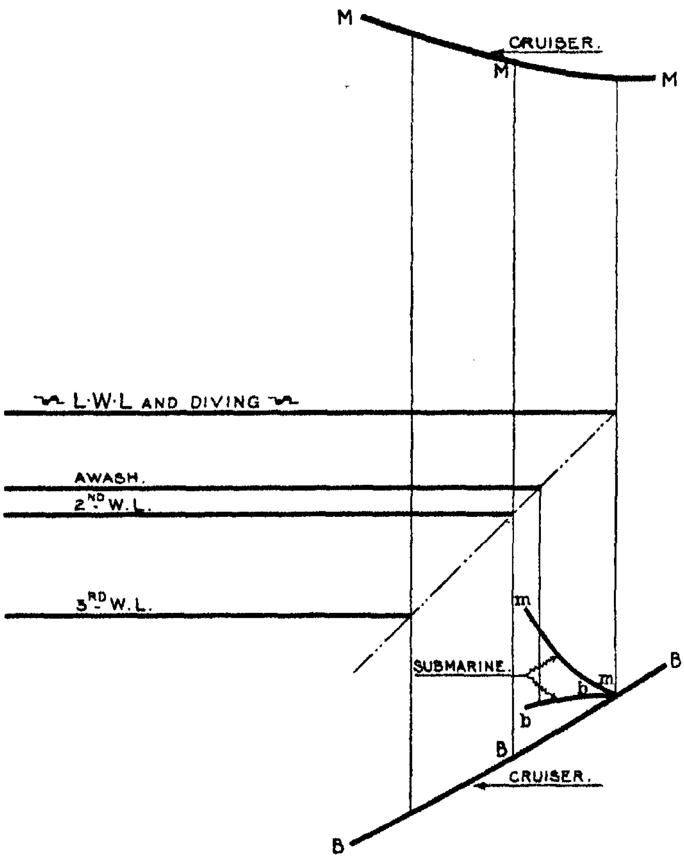


Fig. 3.

in the diving condition it is only 1·25 feet above. For the cruiser floating at a water-line 18 inches below the load draught, the height is 352 feet; at the load draught it is 328 feet. Expressed in terms of the length over all, the heights of metacentres above centres of buoyancy are 0·25 and 0·0083 times the length respectively for the awash and diving conditions, as against 1·35 and 1·26 times the length for the cruiser at corresponding draughts. These

figures indicate the relatively small longitudinal stability of the submarine, and the necessity for avoiding any movements of weights when the vessel is in the diving condition or submerged.

Reference has been made above to the effect upon stability produced by the addition of superstructures. Fig. 4 illustrates this effect for transverse inclina-

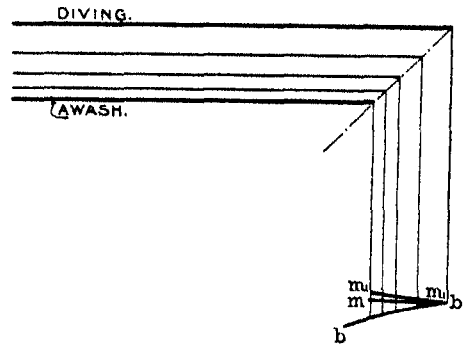


FIG. 4.

tions, *m m m* being the metacentric *locus* without superstructure, and m_1 m_1 m_1 the *locus* with superstructure closed and water excluded from spaces between it and the cigar-shaped hull. In the awash condition the height of the transverse metacentre above the centre of buoyancy is increased about *one-sixth* by the superstructure. The effect of the superstructure upon longitudinal stability is much more marked, as will be seen from fig. 5. In

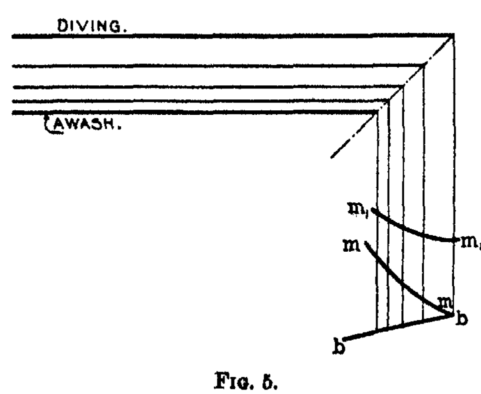


FIG. 5.

the awash condition, closing the superstructure increases the height of metacentre above the centre of buoyancy by fully 50 per cent.

It will be obvious from these diagrams that the maintenance of the full reserve of buoyancy is essential to the safety of a submarine when proceeding

at maximum speed at the surface. In the case of A8, owing to special circumstances, this condition was not fulfilled, and the vessel proceeded at full speed on the surface with her ballast-tanks partly filled with water and with only 6 tons reserve of buoyancy as against the maximum reserve of 13 tons. In consequence of this deeper draught the longitudinal metacentric height was reduced from 12 feet to 8½ feet and the power of resisting changes in longitudinal trim was correspondingly diminished. Since that accident took place, definite orders have been given by the Admiralty that the maximum reserve of buoyancy shall always be secured before submarines are driven at full speed on the surface. The precaution is obviously necessary.

When a submarine is in the diving condition with all apertures closed and crew stationed, the metacentric height (as above stated) is very small, and the trim may be sensibly and rapidly disturbed by small external forces. Consequently very moderate angles of helm given to the horizontal rudders by the operator will produce sensible changes of trim; and, as the pressures on the rudders vary as the *square* of the speed of the vessel, increase in speed with consequent increase in rudder pressures demands greater skill and precaution on the part of the helmsman. A very small amount of trim "by the bow" in association with moderate speed when submerged will bring a submarine to a considerable depth below the surface in a very short time. Experience proves that with trained and disciplined operators at the helm, and with moderate speeds such as have been accepted hitherto, submarines can be worked at fairly constant depths below the surface. On the other hand, many cases have occurred where submarines have reached considerable depths and have touched bottom in consequence of slight accidents or failure in control. These considerations point to the conclusion that much higher speeds than have been obtained hitherto when submerged must be accompanied by greatly increased risk; and it may be questioned if the gain in offensive power, obtained by increased speed, justifies the change in these circumstances. For large submarines it is universally agreed that automatic appliances for regulating depth below the surface are not to be trusted, although they are successful in locomotive torpedoes.

Close approximations can be made to the pressures developed on the horizontal rudders of a submarine moving at a given speed, and to the corresponding changes of trim produced in the vessel. Similar approximations cannot be made at present to the pressures and inclining moments consequent on the stream-line motions in the water surrounding a submarine when she moves ahead. This matter can only be dealt with by direct experiment on models and submarines. In the course of the enquiry into the foundering of A8, this conclusion was universally accepted. Differences of opinion existed

as to the primary cause of that accident. It was obvious that the deeper draught, the lessened stability and the open hatch all conduced to the disaster; but experienced witnesses asserted that they were not of opinion that the vessel could have been made to dive suddenly as she did if she possessed as much as 6 tons reserve of buoyancy. Others equally experienced entertained the opinion that this was the real cause of the accident. After a careful analysis of the evidence the author was convinced that the latter opinion was correct. It was stated at the time that the Admiralty proposed to have experiments made at their experimental tank and on actual submarines in order to settle this difference of opinion. Up to the present time no results of such Admiralty experiments have been published: if they have been made, this silence is much to be regretted on scientific grounds, and no reason is seen for refusing the information. It has been stated authoritatively that experiments of the kind have been made on models of submarines at the Experimental Establishment of the United States Navy at Washington, and that the results have confirmed the opinion expressed by the author.

In connection with the enquiry into the loss of A8 it was made known that her commanding officer recognised the fact that lessened stability must accompany deeper immersion, and that he trimmed the vessel 4° by the stern (lifting the bow about 4 or 5 feet) in the belief that this change would make the vessel less liable to be driven under water by the stream-line action on the bow.

In considering all the circumstances the author was consequently led to investigate the variations in stability accompanying changes of trim in submarines, and to compare them with corresponding changes in other ships. The technical term "trim" here used means the difference in draught of water at the bow and stern: it has no relation to "trimming" for diving. It was obvious, of course, that the cigar-shape must introduce variations in stability with change of trim much greater than those which would occur in vessels of ordinary form, and it was known that in ordinary vessels the changes of trim which occur in service are not of practical importance. Figs. 6 and 7 give the results obtained for the submarine awash and for the cruiser at load draught, when changes of trim take place by the bow and stern, up to 6° from the "even-keel" condition. In order to compare the two types more closely, the heights of metacentres above centres of buoyancy for the even-keel condition are treated as "unity" in both cases, although they differ widely, as above stated. Ordinates to the curves at any angle of trim measure the relative heights of the corresponding metacentre above the centre of buoyancy. Fig. 6 shows these heights for transverse inclinations and fig. 7 those for longitudinal inclinations. In both cases the effect of superstructures is omitted.

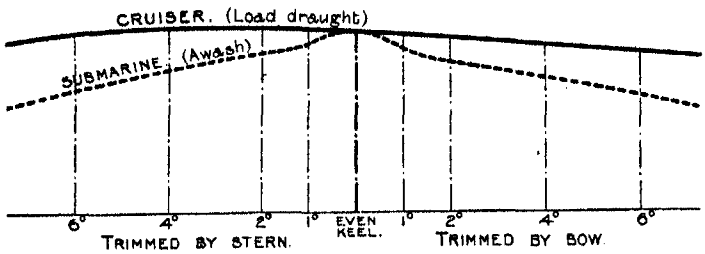


FIG. 6.

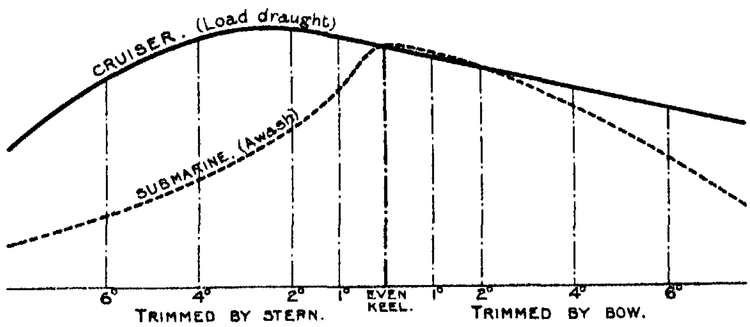


FIG. 7.

Longitudinal stability is more important and the results may be briefly summarised. Taking 4° trim by the stern, the height of the metacentre above the centre of buoyancy in the submarine is only 45 per cent. of the height when the vessel is on an even keel. For the cruiser the corresponding figure is 100 per cent.: that is, there is practically no change in longitudinal stability within the limit of trim mentioned. If the superstructure came into play in the submarine the percentage of the metacentric height at 4° by the stern to the height on even keel would exceed 50 per cent. It will be seen, therefore, that for a cigar-shaped vessel departures from even keel are accompanied by serious decrease in longitudinal stability, and it may be doubted whether the depressing effect of the stream-line motions at the bow would be reduced to an equal extent, if at all, by raising the bow to the extent done in the case of A8. The latter point, however, is determinable only by direct experiment.

Fig. 8 represents three conditions of draught and trim for the submarine dealt with in the calculations.

The foregoing statements lead to the conclusion that in the design of submarines the calculations for stability require to be worked out by naval architects to an extent which is not necessary for ships of ordinary form, and that each departure from precedent must be most closely scrutinised and exhaustively considered. It is true, no doubt, that for the diving and

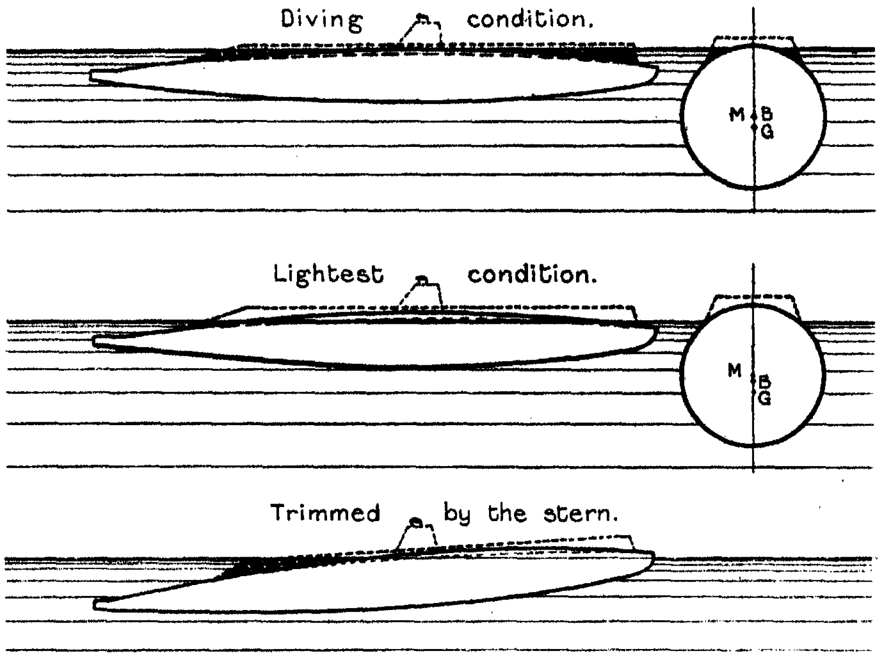


FIG. 8.

submerged conditions the essential point is to deal accurately with questions of weight and position of the centre of gravity, since stability *in all directions* when submerged depends upon the relative positions of the centres of gravity and buoyancy, and moderate " metacentric heights " have to be accepted. On the other hand, it is certain that equal attention should be directed to the conditions of stability in the awash condition, and in the stages of immersion between it and the diving condition. Submarine design is not a task to be entrusted to amateurs or imperfectly informed persons. Skilled naval architects alone should undertake the work, and the results of their investigations should be put into the form of simple practical rules for the guidance of officers and men. From the nature of the case—in consequence of the singular forms of the vessels, the small reserves of buoyancy, and the exceptional variations in stability which must be accepted in order to obtain the power of rapid submergence—considerable risks must be taken. It is, therefore, the duty of all concerned to give all possible assistance to officers and crews in the form of information and instructions based on thorough investigation and experiment.

On a Method of Obtaining Continuous Currents from a Magnetic Detector of the Self-Restoring Type.

By L. H. Walter, M.A.

(Communicated by Professor Ewing, F.R.S. Received March 28,—Read April 5, 1906.)

Magnetic detectors of electric waves can, for the purpose of considering their action, roughly be divided into two classes according as the magnetic mass or core to be acted upon by the electric oscillations set up in the receiving conductor upon the arrival of waves is situated outside the influence of the magnetising field at the time when such oscillations are acting, or is all the time in the magnetic field. In the first case, the energy available as a result of the action of the oscillations is limited to that represented by the remanent magnetism in the core, while in the second case it can be derived in part, though not wholly, from the external field.

Detectors belonging to the first class have been designed which are capable of giving unidirectional currents—practically continuously, as in the case of Fleming's quantitative detector,* or intermittently, as unidirectional impulses, in Marconi's more recent relay-operating detector. Although no details have been published relating to the latter, a cursory inspection of the instrument exhibited at the Royal Institution in 1905 showed it to belong to this class.

Detectors of the second class, in which the magnetic mass is generally either taken through a slowly performed complete cycle of magnetism or else subjected to continuous reversals in a field of constant strength—exemplified respectively in Marconi's cyclic flux and moving band forms of detectors—present other advantages, the chief among which are automatic action and the derivation of part of the energy from the external field. No method of obtaining a continuous current from such detectors has, however, hitherto been devised, such as could be used for rapid recording work, although Tissot has described an arrangement of Marconi's cyclic flux detector by which indications were received on a ballistic galvanometer.† For this reason the use of these self-restoring detectors has up to the present been limited to telephonic reception, the alternating impulses produced as a result of the action of oscillations prohibiting the employment of a relay or recording instrument. This drawback was pointed out by Marconi in his 1905 Royal Institution lecture.

* 'Roy. Soc. Proc.,' vol. 71, p. 398, 1903.

† 'Comptes Rendus,' vol. 136, p. 361, 1903.

In view of the above it was considered that a description of a method by which the author has succeeded in obtaining continuous unidirectional currents from a detector of this type might prove of some interest.

The method was arrived at as a result of experiments in connection with an instrument previously described,* to determine the cause of the increase of hysteresis loss as a result of the action of oscillations. It was found that the increase is due to a great extent if not entirely to the increase of induction produced, to which increased induction a largely augmented hysteresis loss corresponds at the field strength employed. Working on this basis, it was thought that such an increase of induction might serve as a means of furnishing continuous unidirectional currents, by generating a unidirectional (commuted) E.M.F., *i.e.*, by making conductors cut the lines of force in a magnetic field, and causing the oscillations to act upon a magnetic mass undergoing reversals of magnetism in the magnetic field of the generator, whereby the E.M.F. generated should be augmented; a second, equal E.M.F. being opposed to the first, so that normally there is no external potential difference. In such a case a continuous unidirectional current should be obtainable during the time that the oscillations are acting upon the magnetic mass.

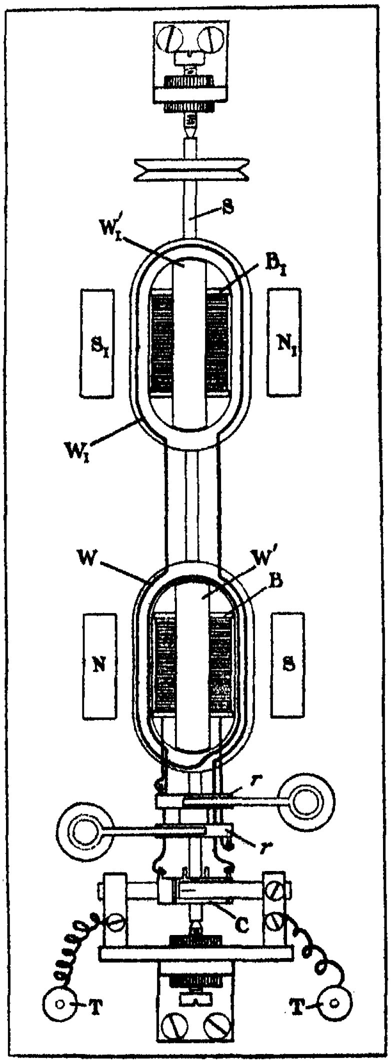
An experimental apparatus was accordingly made, a diagrammatic plan of which is given in fig. 1. Two ebonite bobbins, BB_1, mounted on the same spindle are rotated in the field of two horseshoe permanent magnets NS, N_1S_1, these bobbins being wound, in a similar manner to those illustrated in connection with the pivoted bobbin detector previously referred to, with some feet of steel wire of suitable resistance. A winding of two coils, W,W′, at right angles to one another, of a hundred turns, is placed on each bobbin, at right angles to the plane of the steel wire winding, as in a drum armature, corresponding coils, *i.e.*, W and W_1, W′ and W'_1, being connected in such a way that the E.M.F.'s generated are equal and opposite. The ends of the windings are connected to the segments of a 4-part commutator C. (For the sake of clearness only one pair of corresponding windings, of one turn each, is shown connected in fig. 1.) The steel wire windings of the two bobbins are exactly alike, the ends of one winding being insulated, while those of the other are connected to a pair of slip-rings, *rr*, and brushes, by means of which the oscillations can be passed through the winding.

On testing this apparatus in the normal condition, with the armature driven by a small electric motor, and no oscillations acting, there was no potential difference at the brushes, the zero of a sensitive Ayrton-Mather

* Walter and Ewing, 'Roy. Soc. Proc.,' vol. 73, p. 120, 1904.

galvanometer connected to the terminals TT remaining undisturbed. On waves arriving, a steady deflection on the galvanometer was obtained, in a direction corresponding to an increase of E.M.F. generated by the armature (bobbin) acted upon by the oscillations. On the oscillations ceasing the galvanometer deflection returned to zero. The effect naturally was very small in the first experiments, but it has been found that by suitable designing the magnetic winding and proportioning the turns in the armature winding a quite considerable sensibility is obtained, and this is continually being improved upon. The usual speed employed is about five to eight revolutions per second; higher speeds have been tried and give a larger effect, but the zero is not so steady.

Fig. 1.

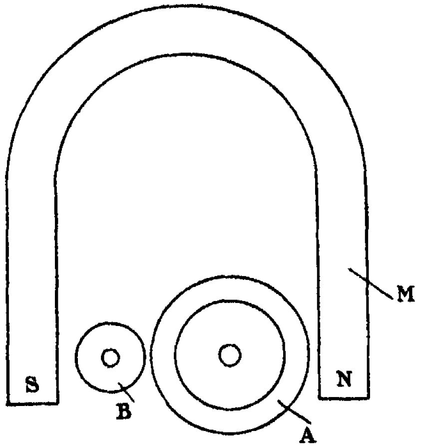


The model illustrated is not adapted to give the best results, this form having been chosen solely for convenience in construction. A considerable length of the winding on the armature is "dead" wire, and hence in a new model being constructed the armatures resemble small Gramme ring structures, in which the wire is more effectively utilised.

The results obtained with the first form of the apparatus led to the idea that the magnetic mass might be located elsewhere in the magnetic circuit of M, such as at B in fig. 2, undergoing slow continuous reversals at the most favourable speed, and an ordinary ring armature A be used, which latter could then be run at a much higher speed so that a proportionately greater external potential difference as a result of oscillations acting could be anticipated, two identical generators opposed to one another of course being employed as in the previous method. The few experiments made in this direction have, however, not given good results up to the present, but this is considered to be due rather to

the experimental apparatus employed than to the inapplicability of the method.

FIG. 2.



Since in many cases it may be desirable to receive signals or indications simultaneously by means of a telephone as well as recording them, a telephonic receiver may be connected so as to take off the current produced as a result of such signals, at some point before it is commuted into unidirectional current, as the alternating current is better adapted for actuating the telephone. When a relay alone has to be actuated, however, it may be advantageous to so arrange matters that the generated E.M.F.'s do not exactly balance, and a small initial current, insufficient to actuate the relay, passes all the time through it. By this means the impulse resulting from the action of oscillations has only to supply little more than the current required to effect the actual movement of the relay tongue or coil, the steady current always passing being sufficient to almost start it from its position of rest. The change can be rapidly effected by a very slight shift of the brushes.

While the author's method of passing the oscillations directly through the magnetic winding leads to a very simple mechanical construction, there is nothing to prevent the older, more general method being made use of, in which they are passed through a separate copper wire winding on the outside of the magnetic core (co-directional oscillations). Since this paper was written the author has seen a proof of a paper giving the results of recent experiments by J. Russell,* on the effect of co-directional and of transverse electric oscillations on the magnetism of sheet iron. These

* 'Roy. Soc. Edinburgh Proc.,' vol. 26, No. 1, 1905–6.

results tend to show, if applied without discrimination, that the iron at the low field employed is more sensitive to the co-directional oscillations; but the experimental conditions as regards the transverse oscillations, which latter are produced in a solid mass of metal, and hence are nearly entirely dissipated in the form of eddy currents, are so entirely different from the circular magnetisation obtained by the author's method, in which the value of the magnetisation at the surface of the magnetic wire carrying the oscillations varies inversely as the radius of the wire, that a comparison cannot be made. A combination of the two methods appears to offer additional advantages, but has not yet been tried.

On a Static Method of Comparing the Densities of Gases.

By Richard Threlfall, M.A., F.R.S.

(Received April 3,—Read May 3, 1906.)

The measurement of small differences of pressure to a fairly high degree of accuracy is not difficult. I have indicated a construction of a micro-manometer* which in its ordinary commercial form has a range of 3 or 4 cm. of height of a liquid column and reads to 0·005 mm. direct. Dr. Stanton† describes a manometer constructed on Professor Chattock's principle, having a reading sensitiveness of 0·0015 mm. of water, but the range is not stated. Lord Rayleigh, observing the contact between mercury surfaces and sharp points, obtained a sensitiveness of 0·0005 mm. of mercury with a range of about 1·5 mm.‡

The idea of employing the micro-manometer for the determination of the relative densities of gases first occurred to me in 1901 in considering the corrections to a set of Pitot tube observations taken in a gas pipe situated some 20 feet above the manometer, and though a rough trial was carried out at the time it is only recently that I have had an opportunity of making an adequate test of the method.

Referring to the figure, A_1 A_2 are the two limbs of the micro-manometer—the liquid (coloured water or oil) being in communication by the syphon B. The gases whose densities are to be compared are allowed to pass into the long tubes C_1 and C_2, through the openings at D_1 and D_2, where there are

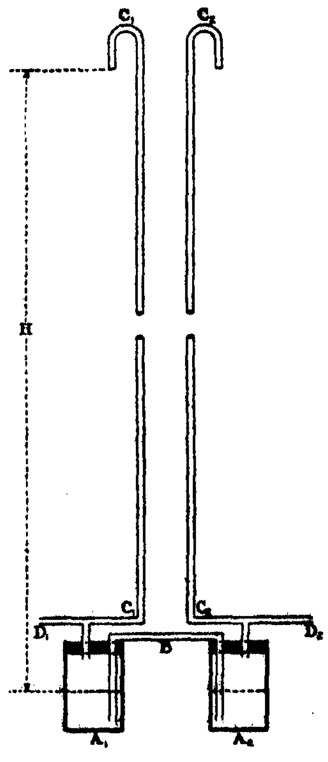
* 'Inst. of Mechan. Engin. Proc.,' February, 1904, p. 273.

† Minutes of 'Inst. of Mechan. Engin. Proc.,' vol. 156, session 1903—1904, part 2.

‡ 'Phil. Trans.,' A, vol. 196 (1901), p. 205.

taps. The tubes C_1 and C_2 are formed of ordinary composition gas pipe and are kept at the same temperature by being placed in an outer iron pipe, not shown—through which a current of water passes—the temperature of the water being ascertained by sensitive thermometers.

It is obvious that since the upper openings of the tubes C_1 and C_2 (bent downwards for gases less dense than air) are in the same horizontal plane, the gaseous contents are exposed to the same external pressure, while the pressure in the limbs A_1 and A_2 corresponds to the external pressure, together with the "pneumostatic" pressure due to the height and density of the columns of the gases in the composition pipes. Let the density of the gas in the pipe C_1 be ρ_1 and of the gas in the pipe C_2—ρ_2, and let H be vertical distance between the free ends of the composition pipes and the surface of the liquid in the manometer when both pipes are filled with the same gas. Let σ be the density of the liquid in the manometer and h the manometer reading, *i.e.*, the difference of level of the liquid in the two limbs due to the density of the gases being different in the two limbs.



If h is very small compared with H, then it is easy to see (small corrections being omitted) that

$$\rho_2 = \rho_1 - \sigma \frac{h}{H}$$

in the case where the pressure at the foot of the tube C_1 is the greater.

The practical limit of accuracy is fixed by the determination of h, and since in the micro-manometer the actual error is independent of the magnitude of h, the experiment must be arranged in such a manner that h is as large as possible.

In the actual experiment a height of nearly 20 metres was available for the gaseous columns.

The accuracy of a comparison is thus easily estimated: suppose that the gases to be compared have about the density of air, that $\sigma = 1$, that $h = 0{\cdot}005$ mm. and $H = 20$ metres, we have

$$\rho_1 - \rho_2 = \sigma \frac{h}{H} = \frac{0{\cdot}0005}{2 \times 10^3} = 2{\cdot}5 \times 10^{-7}.$$

The density of air, however, is about 0·00129 gramme per cubic centimetre under standard conditions, so that the smallest difference of density observable by this method is $1·93 \times 10^{-4}$ of the actual density or, say, 1/5000 part.

Lord Rayleigh and Sir William Ramsay* state that the mean of their determinations of the density of "chemical nitrogen" is 1·2511 and of "atmospheric nitrogen" 1·2572 in grammes per litre.

The difference is thus about 1/2100 of the density of the lighter gas. It appears, therefore, that the static method on the scale described should be able to show the difference between the density of "chemical" and of "atmospheric" nitrogen.

Producer gas being lighter than air, it is advantageous to lead it into the column from the top, so that the air in the pipe may be displaced with as little mixing as possible. A preliminary experiment with a rough and ready piece of apparatus has shown that when hydrogen was passed in from the bottom it was very hard to get the column free of air. It is necessary to protect the free ends of the columns from draughts.

In practice, producer or other gas, after passing a suitable drying system, first displaces the air from a large flask which is in communication both with the column and a loosely plugged test-tube—the connection to the latter being by means of about 1 metre of glass tubing of 2 or 3 mm. bore. After a time the flow to the test-tube is stopped and the column filled—the bottom being left open to the air by slipping the rubber tube off the manometer.

The column itself was erected in the angle of a square brick tower, and the height was measured by means of a steel tape—not a method for a classical experiment, of course, but sufficient for the purpose. The water supply was from a large tank at the top of the tower and the water first passed down the tower so as to attain the temperature of the tower as nearly as possible before entering the jacket of the columns. The composition pipes were ¼ inch internal diameter, and were strengthened and soldered to the flanged plate where they passed through it. The manometer was at such a height that the free surface of the manometric fluid was at practically the same level as the bottom of the water column. Similarly the open ends of the columns are practically at the level of the top of the water—but as the water and tower were at substantially the same temperature, it was not necessary to be very particular.

The comparison of the densities of producer gas and air appeared to offer no difficulty—everything proceeding "according to arrangement." A set of 20 settings of the micro-manometer showed a probable error of the total of

* 'Phil. Trans.,' A, vol. 186, p. 189.

about 0·00077 mm., and of a single setting 0·0034 mm.; in satisfactory agreement with the opinion formed on instrumental grounds that the error of a single setting might be about 1/200 mm. at most.

The value of h, deduced for the sample of gas used—air filling the other column—was 0·3458 cm., the manometer fluid being distilled water coloured by a trace of aniline blue.

Other data were:—

Height of barometer, corrected for temperature	748·6 mm.
Mean temperature both of water jacket and of tower	18° C.
Height of columns of gas above undisturbed level of micro-manometer	1956·8 cm.
Height of manometric column	0·3458 cm.
Density of manometric fluid..	0·9987

Hence density of producer gas

$$0{\cdot}001195 - 0{\cdot}9987\frac{0{\cdot}3458}{1956{\cdot}8} = 0{\cdot}0010185,$$

or, reduced to 0° C. and 760° mm., the producer gas density is 0·001102 gramme per cubic centimetre.

The gas was most carefully analysed several times and the density calculated from the composition; the result was:—

Density = 0·001089 gramme per cubic centimetre,

which agrees with the absolute density as nearly as could be expected from an analysis made with commercial apparatus.

Three days later a second experiment was made on the gas which was being manufactured at that date, and a manometer reading of 0·3550 cm. was observed.

This remained constant for several fillings of the columns, and even after they had remained at rest for several hours. The resulting density in this case was 0·001098 gramme per cubic centimetre.

I have pleasure in acknowledging my indebtedness to my assistant, Mr. Bradbury, who carried out the experiment described above.

A Variety of Thorianite from Galle, Ceylon.

By WYNDHAM R. DUNSTAN, M.A., LL.D., F.R.S., and B. MOUAT JONES, B.A. (Oxon.), Assistant in the Scientific and Technical Department of the Imperial Institute.

(Received April 11,—Read May 10, 1906.)

In a previous paper* an account has been given of the composition and properties of a new mineral from Ceylon, chiefly composed of thorium dioxide (78 to 79 per cent.), to which the name thorianite was given. The mineral was shown to contain, besides thoria, a certain proportion of oxides of uranium varying in the three specimens analysed between 11 and 15 per cent., the uranium being present partly as dioxide and partly as trioxide, that is to say, in the same condition as in uraninite or pitchblende. It was shown that the evidence strongly supports the conclusion that thorianite and uraninite possess the same crystalline form and that they are isomorphous minerals. It is interesting to note that the new mineral naëgite, from Japan, which is essentially a silicate of uranium containing thorium, appears to be isomorphous with thorite (thorium silicate). It was suggested that the intimate association of thoria with oxides of uranium in thorianite is to be regarded as a case of isomorphous mixture. Such a mixture, or perhaps "solid solution," might result from the crystallisation of thorianite from a fused mixture or magma containing the oxides of both elements.

We have now obtained important confirmation of this view through the analyses of some unusually large crystalline fragments of thorianite received from the Galle District of Ceylon, which show that a still wider variation may occur in the proportions of the two oxides present in the mineral. This variety is composed of black lumps, usually of indefinite crystalline form, some of the pieces being apparently cubic. They are often partially covered with a brownish yellow substance containing a considerable amount of silica and probably derived from the associated rock. Some of the pieces weighed over 50 grammes, and were evidently portions of much larger masses. The fractured surfaces often showed a slightly less lustrous and more pitchy appearance than ordinary thorianite. The appearance of the mineral is thus intermediate between the small separate and cubical crystals of thorianite and the large masses without definite crystalline form, which are usually characteristic of uraninite. A small amount of material from Hinidumpattu in Galle consisted of small cubic crystal almost indis-

* Dunstan and Blake, 'Proc. Roy. Soc.,' A, vol. 76, 1905.

tinguishable in appearance from ordinary thorianite. In hardness, optical properties, density and general physical characters this variety of thorianite closely resembles the ordinary form of the mineral.

Analysis by the methods described in the previous paper* on thorianite shows, however, that it contains a much larger proportion (from 27 to 32 per cent.) of oxides of uranium and less oxide of thorium, whilst the minor constituents of the two forms of the mineral are seen to be similar in nature and quantity. It appeared possible that the masses might consist of uraninite crystallised on a nucleus of thorianite or *vice versâ*, or that the two minerals might be separately crystallised within the masses. Analyses of different portions of one and the same piece have, however, given nearly identical results, and the oxides of the two elements appear to be uniformly distributed through the mineral, although there is some variation in the composition of specimens obtained by different collectors.

It may be noticed in the following table that the Analysis I of the small crystals from the Galle District shows rather more uranium and less thorium than that of the larger lumps (II to VI). If the uranium is calculated as UO_2, the molecular ratio of ThO_2 to UO_2 in the small crystals is almost exactly 2 to 1, but there is no evidence that this is more than a coincidence. No. VII was a large crystal, 8 mm.* cube, of ordinary thorianite from Balangoda.

The fact that thoria in thorianite is naturally associated with quantities of oxides of uranium varying from 11 to over 30 per cent., confirms the conclusions, indicated in the previous paper, that the oxides of thorium and uranium are present in thorianite in that intimate association known as "isomorphous mixture."

Different specimens of thorianite from the Galle district have furnished 58·84, 62·16, 62·30, 63·36 and 66·82 per cent. of thoria, whilst common thorianite from other localities has furnished from 76 to 79 per cent. of thoria.

The relations of thorium and uranium in minerals is a subject of some importance in connection with the present developments of the theory of the chemical elements. Attention has been directed recently to the subject by the Hon. R. J. Strutt.† It may be noticed in this connection that the percentages of thoria recorded by Mr. Strutt in his paper have been calculated from the observed radio-activity of the minerals, and not directly determined by chemical analysis. A comparison of the two series of percentages shows differences which in some cases are considerable. The calculated results are, however, only to be regarded as roughly approximate.

* *Loc. cit.*

† 'Proc. Roy. Soc.,' A, vol. 76, p. 88 1905.

The Galle variety of thorianite, as was to be expected, is radio-active, and it contains helium.

Mr. Strutt has kindly examined the radio-activity of this variety, and compared it with ordinary thorianite. The method of comparison was as follows:—"The quantity taken in each case was about one-tenth of a gramme. It was dissolved in strong nitric acid, and most of the excess of acid driven off by evaporation. The solution was diluted and exactly neutralised with ammonia, to prevent any injurious effect of the acid fumes on the apparatus. The solution was then made up to a standard volume.

The solutions of the two minerals were placed in exactly similar test-tube wash-bottles. These vessels were selected to have exactly the same diameter, and the inner tube in each case dipped to exactly the same depth in the solution. A continuous current of air could be drawn through either of them alternately into an electroscope. A two-way stop-cock made it easy to exchange one wash-bottle for the other. Constant suction was secured by the obvious device of letting in air through a deeper wash-bottle attached to a T-piece on the other side of the electroscope. The pressure driving the air through the electroscope was thus equal to the difference of depth between this bottle and the wash-bottle containing the active solution. The current was regulated once for all by a stop-cock, and was not sufficiently rapid to appreciably disturb the gold leaf of the electroscope.

In making the comparisons, air was first drawn through the solutions long enough to expel all accumulated radium emanation. Three measurements of the rate of leak due to the first solution were taken, then three with the other solution; then three more with the first, and so on. The mean of each of these sets was compared with that of its successor. The mean ratio so obtained was corrected for the slight difference in the quantities of material taken. The correction (or normal leakage of the electroscope) was too small to be worth applying. Two samples of each mineral were weighed out, and compared in the manner above described in every combination. Four values for the ratio of thorium activities in the two minerals were thus obtained. They were as follows:—

$$B/A = 1{\cdot}19,\ 1{\cdot}12,\ 1{\cdot}10,\ 1{\cdot}24. \quad \text{Mean, } 1{\cdot}16.$$

The specimen of thorianite B used in this comparison contained 78·86 per cent. of thoria and 15·1 per cent. of uranium oxides. The specimen of the Galle variety A used was that numbered I in the table of analyses. The results of the radio-activity determinations are in fair accordance with the proportions of uranium and thorium determined by analysis."

In the following table of analyses, I represents the composition of the small crystals from Hinidumpattu, Galle District; II to VI represent the compositions of the large lumps from the same locality; II, III, and IV are analyses of different parts of the same crystal, III being that of the outer layer; VII is an analysis of a large crystal of ordinary thorianite from Balangoda.

Table of Analyses.

	I.	II.	III.	IV.	V.	VI.	VII.
ThO_2	58·84	62·16	66·82 (ThO$_2$ and (Ce,La,Di)$_2$O$_3$)	—	62·32	68·36	78·98
$(Ce,La,Di)_2O_3$	0·85	1·84		—	2·24	1·16	1·47
Y_2O_3	—	—	—	—	—	—	--
UO_2	32·74 (UO$_2$ and UO$_3$)	10·32	28·24	28·68	27·02	27·99	13·40
UO_3		18·88					
PbO	2·56	2·29	2·29	2·50	2·99	2·90	2·54
Fe_2O_3	1·31	1·11	1·22	2·43	2·28	1·27	0·87
CaO	0·19	0·59	0·54	—	0·50	0·85	0·91
H_2O	1·26	1·05	1·00	—	2·16	1·32	1·28
Insoluble in nitric acid	0·45	0·77	0·56	0·54	0·87	0·77	0·47
He, CO_2	present	present	present	present	present	present	present

Mineralogists have often assigned a new name to a mineral found in a new locality when it has differed essentially in composition from the previously known mineral. It does not, however, seem desirable, in the light of present knowledge, to regard this variety of thorianite from Galle as a new mineral species, especially when it is remembered that the oxides of thorium and uranium in thorianite are not chemically combined and that from the probable mode in which the mineral has been formed, it is to be expected that considerable variations would naturally occur in the proportions of the two oxides in the mineral found in distinct localities which have therefore crystallised under different conditions. In fact, it is probable that further examination of other specimens from distinct localities may reveal the existence of a series of substances intermediate between the hypothetical pure uraninite, consisting of uranium dioxide, and the pure thorianite, consisting of dioxide of thorium.

Some Stars with Peculiar Spectra.

By Sir NORMAN LOCKYER, K.C.B., F.R.S., LL.D., Sc.D., and
F. E. BAXANDALL, A.R.C.S.

(Received May 5,—Read May 17, 1906.)

In a paper on the chemical classification of the stars* communicated to the Royal Society on May 4, 1899, it was pointed out that it was then possible to classify the stars according to their chemistry. In a later publication† the spectra of the brighter stars were arranged in groups according to the suggested classification.

In the course of this work it was found that in the case of a few stars the spectra show certain peculiarities, and do not altogether conform to any common type. The most notable of these stars are α Andromedæ, θ Aurigæ, α Canum Venaticorum, and ϵ Ursæ Majoris. These are all on the descending side of the Kensington curve of stellar temperature, the first three being of the Markabian type and the last of the Sirian type. The present paper contains a short account of their spectra. More minute discussion will be reserved for a subsequent memoir.

α Andromedæ.

This star has been recently found by Slipher, of the Lowell Observatory, to be a spectroscopic binary. In the published statement‡ to that effect, the period is given as about 100 days. This result has been based on measures of the displacement of the H_γ and Mg 4481·3 lines from their normal position. The Lowell publication does not give an account of the general nature of the stellar spectrum, and there is no mention of any changes in the relative intensity of any of the lines.

Prior to this announcement of Slipher, an investigation of various spectra of α Andromedæ, taken between the years 1900—1904 at Kensington, appeared to indicate slight changes in the relative intensity, position, and definition of some of the lines in the various photographs.

In the classification α Andromedæ was placed in the Markabian group, the accepted type star for which is α Pegasi (Markab). The Markabian stage is on the descending side of the temperature curve immediately higher than the Sirian stage, and below the Algolian (β Persei). Although placed in the Markabian group, α Andromedæ, as determined by the behaviour of the

* 'Roy. Soc. Proc.,' vol. 65, p. 186.

† 'Catalogue of 470 of the Brighter Stars,' published by the Solar Physics Committee.

‡ 'Lowell Observatory Bulletin,' No. 11.

helium lines in its spectrum, represents a slightly higher stage, but it approaches more closely to the type star of the Markabian group (α Pegasi) than to that of the higher temperature Algolian group (β Persei). Its acceptance as a Markabian star was based on the behaviour in its spectrum of the lines of hydrogen, helium, magnesium, silicium, etc.

Next to the lines of hydrogen and helium, the most prominent lines which have been traced to known terrestrial elements are those of silicium Group II ($\lambda\lambda$ 4128·20, 4131·04), proto-magnesium λ 4481·3, and proto-calcium (λ 3933·83). The strongest enhanced lines of iron, chromium, carbon, and titanium are present, but only occur as comparatively weak lines. In addition to these lines of known origin there occur several strange and well-marked lines, not found in the spectrum of any other star yet examined, and for which no satisfactory terrestrial origin has yet been found.

Of the strange lines, those at $\lambda\lambda$ 3943·9, 3984·1, 4137·0, 4206·3 and 4282·4 are the most prominent. All except λ 4206·3 agree closely in position with fairly strong solar lines, one (λ 3944·1) ascribed by Rowland to Al, the others to Fe, but there is no evidence obtainable from the Kensington Laboratory spectra that these lines of aluminium and iron behave specially under varying conditions and the approximate agreement in wave-length is possibly a fortuitous one.

As the strongest stellar lines, apart from those of hydrogen, are all enhanced lines of certain metals, it would also appear probable that the strange lines mentioned are due to some element or elements not yet found terrestrially or for which, if found, there is yet no record of enhanced lines. The records of other celestial spectra, such as those of nebulæ, bright-line stars, and novæ, have all been searched for possible identification of some of their lines with the strange lines in α Andromedæ, but with no success.

Although, as has been stated, there appear to be slight changes in relative intensity, position or definition of some of the lines in the α Andromedæ spectrum, there does not seem to be any regularity in the changes, either in the lines themselves or in the manner in which they are affected, so that at the present stage it is not possible to come to any conclusion as to their real significance. Whether the changes have any relation to the period established by Slipher cannot be settled from the existing photographs, and additional photographs of the stellar spectrum at extended intervals will be necessary to throw more light on this point.

θ Aurigæ.

Like α Andromedæ, this star has, in the Kensington classification, been placed in the Markabian group, the type of which is α Pegasi. Its spectrum

resembles that of α Pegasi more closely than that of either β Persei, the type star of the Algolian, the next higher group, or Sirius, the representative of the next lower group (Sirian). The θ Aurigæ spectrum, however, lacks the helium line 4471·7 and its real position is intermediate to α Pegasi—in which the helium line mentioned is weakly represented—and Sirius, in which the helium lines are lacking.

Apart from the hydrogen lines, the chief feature of the spectrum of θ Aurigæ is the prominence of the lines of silicium Groups I and II, the behaviour of which in the silicium spectrum has been discussed in a previous paper.* The proto-magnesium line 4481·3 is moderately strong, but the lines of proto-calcium, proto-iron, proto-titanium, proto-strontium, proto-chromium, although present, are comparatively weak.

In addition to the lines of known origin, there are a few which appear to be special to this spectrum. The stronger of these are near λλ 3954·3 (3—4), 4076·3, 4191·8 (2—3), 4200·7 (2—3), and 4377·0 (3). These lines form an entirely different set from the strange lines in the spectrum of α Andromedæ.

Reference to records of terrestrial spectra has afforded no satisfactory results as to the origin of these lines. One of them (λ 4200·7) apparently agrees in position with one of the series of lines discovered by Professor Pickering in the spectrum of ζ Puppis. In the absence of the other lines of the series, however, it is scarcely likely that the line is of identical origin in the two cases.

α Canum Venaticorum.

This star has also been placed in the Markabian group in the Kensington classification, and its general spectrum is very similar to that of θ Aurigæ, one of the stars previously discussed.

Of this stellar spectrum, Pickering remarks that the K line of calcium is extremely faint, and the lines 4128·5, 4131·4 (subsequently traced to silicium Group II) are stronger than in the normal spectra of the same class. Some of the fainter lines, he says, appear to be of "peculiar wave-length." All these abnormalities have been confirmed by an investigation of the Kensington spectra of this star. The lines in the spectrum are, in general, only faint, the most prominent, apart from those of hydrogen, being the silicium lines previously mentioned and the enhanced magnesium line 4481·3. The more pronounced enhanced lines of iron, titanium and chromium, are present, but weak. The ordinary metallic arc lines, which occur prominently in the lower type stars, are lacking.

* 'Roy. Soc. Proc.,' vol. 67, p. 403.

Some of the strange lines—that is, lines not in the normal spectrum of the same general type—appear to be identical with those in θ Aurigæ. Thus there are lines whose wave-lengths are approximately 3954·3, 4076·5, 4136·3, 4192·0, and 4376·8, which occur in θ Aurigæ, but not in the normal spectrum. There are no lines of considerable intensity special to α Canum Venaticorum, though some of the fainter lines seem to be peculiar to this spectrum.

ϵ *Ursæ Majoris.*

This star has, in the Kensington classification, been placed in the Sirian group, the type star of which is Sirius. From the intensity of some of the metallic lines it is apparently somewhat lower on the temperature curve than the Sirian, coming between that and the Procyonian type. Its spectrum, however, resembles the Sirian more than the Procyonian.

The spectrum has been carefully compared with that of Sirius, and any differences in intensity or position of the lines noted. These will be given in detail in a future paper. What peculiarities there are, however, are chiefly confined to alterations in relative intensity of certain lines, very few lines having been found which do not occur in the Sirian spectrum. The spectrum does not, therefore, diverge from the normal type so much as in the case of α Andromedæ and θ Aurigæ.

The most noticeable features of the comparison of this spectrum with the Sirian spectrum are the weakening of the Group II silicium lines at λ 4128—4131, and the strengthening of the enhanced lines of chromium in the former spectrum. Certain of the enhanced lines of titanium and iron seem to be affected, but, as a class, the lines of neither of these elements are affected so much as those of chromium. There are a few cases of lines occurring in ϵ Ursæ Majoris which appear to be lacking in Sirius, but these are nearly all weak lines.

The photographs of the stellar spectra were all taken with one 6-inch Henry objective prism of 45° angle. The dispersion is such that the distance between H_ϵ and H_β is 1·85 inches or 4·6 cm. The various photographs involved in the discussion were obtained by Messrs. Baxandall, Butler, Rolston, and Moss.

ERRATUM.

Page 289, line 20 from the top, instead of "10, 15, 20," *read* "20, 15, 20."

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END OF THE SEVENTY-SEVENTH VOLUME (SERIES A).

Harrison and Sons, Printers in Ordinary to His Majesty, St. Martin's Lane.